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A

PHILOSOPHICAL

INQUIRY

INTO THE CAUSE OF

ANIMAL HEAT.

Eng.

A

PHILOSOPHICAL

INQUIRY

INTO THE CAUSE OF

ANIMAL HEAT:

WITH

Incidental OBSERVATIONS

ON SEVERAL

PHISIOLOGICAL AND CHYMICAL QUESTIONS, connected with the Subject.

BY P. DUGUD LESLIE, *M.D.* K

Desine quapropter novitate exterritus ipsa,
Expuere ex animo rationem: sed magis acri
Judicio perpende, et, si tibi vera videtur,
Dede manus: aut, si falsa est, accingere contra.

LUCRET.

LONDON:

Printed for S. CROWDER, Paternoster-Row; and J. ROBSON, New Bond-Street.—A. GORDON, and C. ELLIOT, Edinburgh.

M, DCC, LXXVIII.

To the Right Honourable

LIEUTENANT-GENERAL

Sir JOHN IRWINE, *K. B.*

COMMANDER IN CHIEF OF HIS MAJESTY'S FORCES IN THE KINGDOM OF IRELAND, *&c. &c. &c.*

THIS TREATISE IS,

With the greatest Respect,

INSCRIBED,

By his most obedient and obliged

humble Servant,

DURHAM,
Dec. 10,
1778.

P. DUGUD LESLIE.

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PART II.

INTRODUCTION.

THE absurd debates, and abstruse speculations of the schools are now no more, and researches into nature are become the business, or the amusement, of the learned and ingenious. The philosophical romances too, for so the systems of many philosophers may be called, are no longer relished by the public; and the lovers of science disdain theories unsupported by facts.

Whilst philosophers scorned to present to the world any thing, but compleat systems of nature, the progress of useful knowledge was necessarily slow, and uncertain; for few had courage to attempt so vast an enterprise, and fewer still were capable of forming an hypothesis adequate to explaining plausibly all the operations of nature. The infinite variety of natural objects, the stupendous coincidence and diversity by which all agree and all differ, must convince us, that no vigour of judgment, no warmth of fancy, are equal to the tracing of every phenomenon to its first cause; and hence our regret, that those talents should have been lost to mankind by speculating on universal nature, which might

have been employed with prodigious benefit in ſome particular branches of philoſophy.

But the mind, for wiſe purpoſes, was formed to deſire the completion of whatever is begun; and from that tendency, the philoſophy ſprang, which deſpiſing the laborious method of inveſtigation and experiment, pretended to explain, by clear ideas, the whole conſtitution of things. This was the Carteſian ſcheme, and the grand ſource of all the ridiculous tenets, and wild notions of that ſchool.

Whoever hopes to acquire an accurate knowledge of the operations of nature, muſt be contented to proceed by a cautious and painful analyſis; for as well might we pretend to build without materials, as to form, without obſervation and experiment, a rational ſyſtem of natural ſcience. The ancients paid little attention to experimental philoſophy, but devoted themſelves, with a truly philoſophical ardour, to the obſervation of the phænomena of nature; and that proceſs was conſonant to ſound reaſon, for experiment is only properly called in, to fill up thoſe chaſms, which ſimple obſervation neceſſarily leaves. The department of experimental philoſophy is the unfolding of thoſe phænomena, the cauſes of which unaſſiſted reaſon cannot perfectly diſcover, and whoſe connexions it cannot trace; and the advancement of that branch of ſcience by conſequence depends

pends on the number and accuracy of our obſervations with reſpect to the relations, which natural objects have to each other.

A philoſopher then ſhould be occupied with enumerating connected objects; with exhibiting their points of coincidence, by eſtabliſhing the analogies by which thoſe effects, whoſe agreement appears ſlight, are to be attributed to the ſame cauſe; and in ſhort, with ſhewing the diverſity ſubſiſting between the moſt ſimilar objects, and the ſimilarity between the moſt different. The Phyſiologiſt in particular ſhould remember that his buſineſs is to narrate facts, not to invent them; that his judgment ſhould only be employed to combine and generalize obſervations, in order to eſtabliſh one univerſal truth, on an uninterrupted chain of facts and probable deductions. I ſay probable, for it would be abſurd to require demonſtration of any truth, not mathematical.

If we conſider the ſtate of medicine from the days of Hippocrates, to the preſent time, we ſhall find that when phyſicians conſulted nature and inſtituted experiments, they made ſome progreſs in uſeful knowledge; but that, when they pretended to ſubſtitute conjectures for facts, they only involved themſelves in a cloud of words, and a labyrinth of errors. The application of the Ariſtotelian philoſophy, by Galen and his diſciples, both to the theory and practice of phyſic, is the cleareſt

proof of this aſſertion; for from the time the Galenical ſyſtem began to prevail, to the beginning of the 16th century, idle ſpeculation, and recondite jargon, were all that could be learned in the ſchools of phyſic.

At this period, Paracelſus, the noted Alchymiſt, a man of an ardent and enterpriſing temper, boldly freed himſelf from the trammels of authority, and ſtruck out a new path for the ſons of Æſculapius. He inveighed with acrimony againſt the pharmacy of the ancients, and in a paroxyſm of madneſs, burnt publicly the writings of the old Greek and Arabian phyſicians. His enthuſiaſm, though extravagant, was eventually of advantage to the healing art. It rouſed the philoſophic from the lethargy in which they had ſo long remained, and generated that active ſpirit of enquiry, which has proved ſo favourable to the advancement of every liberal art.

Vanhelmont, and the other followers of Paracelſus, inherited all his enthuſiaſm, and laboured inceſſantly to explode the ſyſtem of Galen. From their time, chymiſtry, which had been employed as an auxiliary in the cauſe of medicine, began to aſſume an univerſal ſway, and to explain, on the principles of fermentation and mixture, every phenomenon in animated body. But the love of novelty, which both creates and deſtroys theories,

theories, suffered not the illusion in favour of this opinion, long to prevail.

Chymists, when they extended their views, and contemplated universal nature, discovered that they had gone too far; and that the Hermetick art, how unbounded soever in its energies and principles, is inadequate to the explaining of many of the functions peculiar to the animal economy. This discovery we owe to the physicians of the last century; and their conduct gives us reason to deplore the weakness of the human mind, which can rarely avoid one error, without running into another. No sooner had they perceived that all the functions of the animal body could not be expounded on chymical principles alone, than they rashly attributed that universality to the mechanical system, of which the chymical was deprived.

Considerable progress had been made in the study of mechanics, and those who devoted their time to it, found daily new motives to convince them, that, on mechanical principles, many of the arcana of nature might be resolved. Medicine in particular derived no small advantage from having these principles applied to it; and reasonings on them, achieved the ruin of the doctrine of Galen, which chymists had so happily begun. From the same source descended a more enlarged spirit of observation, and greater clearness and precision in medical ratiocinations;

but

but physicians soon became intoxicated with the conceit, that all the phænomena of the animal economy were explicable on mechanical principles, and in consequence of it, wrested every fact, how dissonant soever, to a conformity with that notion.

The animal body however admits not the solution of all its phænomena on any particular set of principles. Various changes are evidently induced in its fluids by chymical and mechanical causes, but it is not less certain, that many functions peculiar to life are regulated by laws steady and uniform in their operation, which are not dependent on any principles of mechanics, or chymistry, hitherto acknowledged.

The judicious physician, therefore, unseduced by any sect or system, should have truth only for his end, regardless whether it be obtained from the crude experience of the Empirick, or the systematick labours of the dogmatist.

But least the patience of my readers should be exhausted, it is time to give some account of the following treatise. The substance of it was published at Edinburgh, in an inaugural thesis, in the year 1775. As at that time, I had no other view than to comply with the institutions of the university, it was written in haste, and sent to the press in much too crude a state for public inspection. Yet in spite of those disadvantages, the importance

tance of the ſubject, and the novelty of the theory, attracted the notice of ſome ingenious phyſiologiſts, whoſe deſire to ſee it treated more at large, and with greater perſpicuity, has induced me to offer it to the public, under its preſent form.

In the proſecution of my plan, I have ſometimes deſcanted on chymical and phyſiological queſtions, which ſeem not, at firſt, much connected with my ſubject; but it has been ſeldom done, and only when ſuch diſſertations tended to elucidate ſome of the radical principles of the theory. Beſides, the ardent deſire I have to excite the curioſity of phyſiologiſts, and to prevail on them to inveſtigate more particularly the phlogiſton, as a very important principle in the animal body, will, it is hoped, be my excuſe, if too much time has been beſtowed in tracing that univerſal agent, through the various phænomena of nature.

The reader cannot fail obſerving that in general, I have evinced the truth of my hypotheſis by experiments inſtituted for different purpoſes, by others; and my reaſon for that procedure is the proneneſs of every one to draw falſe concluſions from experiments, that he makes to confirm a favourite doctrine. Direct experiments, the nature of the ſubject precluded; but there are many in the courſe of this eſſay, which afford a collateral proof of the juſtneſs of the theory.

To

To ſome it may appear, that I have dwelt too long on the hypotheſes of former phyſiologiſts; but it ſhould be conſidered, that a hiſtory of the miſtakes of writers on any ſubject, is perhaps not leſs uſeful, than an account of the truths, which they eſtabliſhed; for, whoever knows the commencement and extent of a deviation from any path, muſt neceſſarily know the path that is deviated from. It was alſo requiſite to give a conciſe hiſtory of all that had been advanced on the ſubject of animal heat, and to aſſign the reaſons, which perſuaded me to reject the prevailing opinions with reſpect to its cauſe, before I ſhould proceed to offer that notion of it, which to me, at leaſt, ſeems better founded.

But of this, my reader will determine, and to his judgment I ſhall chearfully ſubmit; not, however, without ſoliciting his candour and indulgence for the many overſights and imperfections he may diſcover in me, both as a writer and a philoſopher, and intreating him to remember, that a generous allowance for the faults of a young author is the beſt encouragement to genius.

A PHI-

A PHILOSOPHICAL ENQUIRY INTO THE CAUSE OF ANIMAL HEAT.

CHAP. I.

Of ANIMAL HEAT *in general.*

HEAT is in the universe the chief cause and principle of activity: By it, nature carries on her most important operations. Plants and animals derive from that source their growth and vigour; and nature, equally simple and perfect in every energy, has endowed the

animal

animal body with the power of generating its own heat. Without this, the temperature of the air in various climates would have been deſtructive of life, and man could only have been the inhabitant of thoſe zones, which are called temperate.

A queſtion ſo curious as that, which has for its object the cauſe of the power abovementioned, claimed the attention, and exerciſed the ingenuity of the learned and philoſophick of every age; but all their efforts have not yet been able to extricate it from obſcurity, and their explanations have only increaſed the darkneſs, which they were meant to diſſipate.

The ancients poſſeſſed not the requiſites for minutely inveſtigating the ſcience of nature; and prone to ſuperſtition, attributed every phænomenon, which eluded their inveſtigation, to the influence of a ſupernatural power. That proceeding, how unphiloſophical ſoever it may ſeem, was hardly more frequent among poets, than phyſicians; and even Hippocrates, the father and founder of the healing art, when he could find nothing analogous to the generation of animal heat, accounted it a myſtery, and beſtowed on it, many attributes of the deity. In treating

ing of that ſubject, he ſays in expreſs terms, "what we call heat appears to me to be a "ſomething immortal, which underſtands, "ſees, hears, and knows every thing preſent "and to come *."

With reſpect to Ariſtotle, who was too profoundly verſed in the ſcience of nature to attribute the generation of animal heat to a ſupernatural cauſe, we find nothing in his writings, though he ſeems to have given particular attention to the queſtion, that can be ſaid to throw light upon it.

Galen, the author who next claims our attention, ſeemed calculated to promote the advancement of natural knowledge. He had received from nature an acute and comprehenſive genius, which was in every reſpect capable of the higheſt cultivation; but having unfortunately applied very early to the Peripatetick philoſophy, he imbibed all the abſurd tenets, and metaphyſical ſubtilties of that ſchool, and in an evil hour introduced them into the theory and practice of medicine. With reſpect to the cauſe of animal

* Δοκέει δέ μοι, ὃ καλέομεν θερμὸν, αθανατόν τε εἶναι, καὶ νοεῖν παντα, καὶ ορῆν καὶ ακύειν καὶ ειδέναι παντα τε τα οντα, και τα μελλοντα ἔσεσθαι—Περὶ Σαρκων.

 heat,

heat, he tells us, that the diſpute between the philoſophers and phyſicians of his time was, *" whether it depended on the motion " of the heart and arteries, or whether, as " the motion of the heart and arteries was " innate, heat was not likewiſe innate *." Both theſe opinions, however, he rejects, and attempts a ſolution of the queſtion on his favourite ſyſtem; but his leading principles being erroneous, his deductions are of courſe inadmiſſible.

Upon the whole, it is apparent that the ancients were entirely in the dark with regard to the cauſe of animal heat; and though a ſounder philoſophy has enabled the preſent age to throw off the yoke of ſuperſtition, and to explain natural phænomena by natural cauſes, yet it is to this day conſidered by many ingenious philoſophers, as a problem never to be reſolved. Such an opinion, however, ought to be entirely reprobated; for though it may, on a ſlight view, ſeem to ariſe from that philoſophical diffidence, which promotes the inveſtigation of natural cauſes, yet it is the offspring of ſelf-conceit, and may

* Πότερα δὲ ἐκ συνήσιως αὐτὸ πᾶς κατὰ τὴν καρδίαν, και τας ἀρτηρίας ἐχειν τὴν γένεσιν, ἢ καθάπερ αὐτο το κινεῖσθαι τῇ καρδίᾳ σύμφυτον υπαρχει, τὸν αὐτον τροπον και ἡ θερμασια—Adverſ. Lyc. *cap.* II.

justly

juftly be afcribed to our confidering every queftion that has hitherto eluded our inveftigation, as above the reach of human nature. It is the characteriftic of true modefty to make us examine every circumftance with the utmoft care, and to prevent us from deciding dogmatically; always thinking that fome particular unknown to us may generate a different conclufion, and develope what is now involved in doubts innumerable. Let not therefore the thorns and briers, which interrupt the path of fcience, prevent us from proceeding; but let them only make us cautious in choofing our footfteps, and in tracing out the fmootheft tract. Whatfoever is good can only be obtained by labour; and fcience, like virtue, difdains the flothful and negligent.

But to return to my fubject: As it is not a matter of mere fpeculation, but of real importance to thofe, who undertake the cure of difeafes, to be acquainted with the phænomena and caufe of every function peculiar to life, it is the indifpenfable duty of all, who profefs the healing art, to contribute their endeavours to throw light on thofe queftions relating to the animal economy, which have either been neglected by former phyfiologifts,

or

or have evaded their investigation. That consideration, and the high satisfaction attending the study of nature, induced me to undertake the subsequent essay on the cause of animal heat; and though the difficulty of the subject, and a diffidence of my own abilities, make me despair of treating it, as I should wish, yet I trust that an hypothesis shall be advanced, by which a more satisfactory explanation of many things relating to the question will be given, than has hitherto been communicated to the public. The discourse is to be arranged in the subsequent manner.

1*st*. An account shall be given of the phænomena peculiar to animal heat.

2*dly*. The opinions which have prevailed on that subject, shall be explained, and the reasons assigned, which induce me to reject them.

And *lastly*, a new hypothesis shall be advanced, and supported by various arguments.

CHAP.

CHAP. II.

Of the Phænomena of Animal Heat.

IT is obvious, and has been generally admitted by the physiologiſts of all ages, that the animal body is endowed with a power of generating its own heat. To ſupport this poſition by many arguments would be a treſpaſs on the patience of my readers, it ſhall therefore be aſſumed as a fact, and illuſtrated only with ſuch reaſons as throw light on the phænomena peculiar to animal heat.

If a thouſand different inanimate bodies, heated to various degrees, be brought together in a place, where there is no poſitive cauſe of heat, the heat will immediately begin to flow from the hotter to the colder bodies, till all become of one temperature, or to what ſome philoſophers chooſe to call an *æquilibrium* of heat. But this is by no means the caſe with reſpect to animated matter; for whatever be the degree of heat peculiar to individual animals, they preſerve it ſtable and unchanged in every temperature, to which

which they can be expoſed; provided that it be not altogether incompatible with life or health. Thus we find that the human body is not only capable in certain circumſtances, of ſupporting, without any material change, a degree of heat, in which the thermometer riſes conſiderably above that of boiling water *, but likewiſe, that it maintains its uſual temperature, whilſt the ſurrounding medium is ſeveral degrees below the point of congelation.

It is therefore evident, that animals neither receive their heat from the bodies around them, nor ſuffer, from the influence of external circumſtances, any material alteration in that heat, which is peculiar to their nature. This general fact is further elucidated and evinced by many late accurate and well authenticated obſervations, which ſhow that the degree of heat in the ſame genus and ſpecies of the more perfect animals, continues very uniformly the ſame, whether they be environed by mountains of ſnow, in the neighbourhood of the pole, or expoſed to a vertical ſun, in the ſultry regions of the torrid zone.

* Philoſoph. Tranſ. vol. LXV.

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The ſtability and uniformity of animal heat under ſuch a diſparity of external circumſtances, and ſo vaſt a latitude in the temperature of the ambient air, leave no room to doubt that the living body is furniſhed with a peculiar mechaniſm, or power of generating, ſupporting, and regulating its own temperature; and that this is ſo adapted to the circumſtances of the economy, or, perhaps to ſpeak more accurately, ſo immediately dependant upon them, that whatever be the heat of the atmoſphere, it ſhall have very little influence either in diminiſhing or increaſing that of the animal.

S E C T. I.

The Latitude in the Temperature of Animals is conſiderable.

The degree of heat found in different animals is extremely various; but according to the moſt accurate obſervations, all animals are one degree at leaſt warmer than the ordinary temperature of the element, which they inhabit *.

* Martin on therm.

Before the introduction of the Linnæan System, it was usual among zoologists to class animals according to their respective temperatures. The heat of the human body was fixed upon as the standard, and whatever animals either attained to the same degree of heat, or exceeded it, were stiled *hot*, and those that came short of it, *cold*. But as these terms are merely relative, and the qualities denoted by them run into each other without any precise distinguishing limits, it is obvious that this is both an inaccurate and unphilosophical distinction, and ought therefore to be entirely laid aside.

The gradations of life, from the warmest animals, down to perfectly inert matter, are so extremely small, and so difficult to be ascertained, that both zoologists and botanists have been much puzzled in fixing the boundaries between the highest vegetables, and the lowest animals *. I formerly, on this account,

* The latent transition of nature from one genus to another seems to have been early observed by philosophers.—Πολλαχοῦ γαρ ἡ φύσις δήλη γινεται κατα μικρον μεταϐαινουσα ὡς᾽ ἀμφισϐητεῖσθαι ἐπί τινων, ποτερον ζωον ἤ φυτον. "Nature, in many instances, appears to make her transition by little and little, so that in some beings it may "be doubted, whether they are animal, or vegetable." *Themist.* See also *Arist. de Animal. Part.*

account, ſuggeſted *, that where neither the habit, appearance, nor manner of production, afford ſufficient marks of diſtinction, the circumſtance of temperature alone would be found a ſure criterion of the preſence, or abſence of a living principle, and enable us to range beings in their reſpective kingdoms.

Lord Bacon's experiments, which plainly tend to ſhew that no vegetable, any more than the moſt inert and lifeleſs maſſes of matter, is poſſeſſed of a degree of heat beyond the temperature of the ſurrounding medium, led me into that opinion ‡; but of late ſome ingenious experiments have been publiſhed by Mr. John Hunter, from which it appears, that living plants have really a power of reſiſting for a certain time, the communication of cold: or at leaſt they, from ſome latent cauſe or other, are conſiderably longer in being frozen, than dead vegetables ‖. If this be really the caſe, Lord Bacon's opinion muſt

* Diſſert. inaug. 1775.

‡ "In vegetabilibus et plantis nullus reperitur caloris "gradus, neque in lachrymis ipſorum, neque in me- "dullis recenter apertis." *Nov. org. Scient.*

‖ Philoſoph. Tranſ. *vol.* LXV.

fall to the ground, and consequently all conclusions deduced from it: But as the ascertaining of the limits between the animal and vegetable kingdoms is not merely a matter of speculation, but of real importance to naturalists, the subject should be more accurately investigated, and more experiments instituted before the method, which I proposed, be entirely abandoned.

The next thing to be considered, is the different temperature peculiar to the different classes of animals. Those animals, which are furnished with two ventricles of the heart, and are under the necessity of breathing, as Man, Birds and Quadrupeds, are by much the warmest.

The Amphibia are a tribe of animals, whose organization renders respiration less necessary to the support of life. They are indeed provided with lungs similar to those of man; but they continue to have the *foramen ovale* open through life, which in him is generally shut up at birth. They likewise differ from man in this, that some of them have but one ventricle of the heart, some two, and others three *; but they seem all to agree in having

* Mem de l'Acad. des Sciences.

ving the *foramen ovale* open, and on it, in a great meaſure, depends the power which they have of living for a conſiderable time under water *.

The degree of heat in this claſs of animals is much inferiour to that of man, birds and quadrupeds, and ſeems capable of conſi-

* I cannot help taking notice here of an aſſertion of the learned author of 'The origin and progreſs of language.' In comparing the natural capabilities of man with thoſe of brutes, he maintains that man may, by education and culture continued for many years, be transferred almoſt into an animal of another ſpecies, inſomuch that though he be undoubtedly by nature a terreſtrial animal, yet he may be ſo accuſtomed to the water, "as to become as perfectly amphibious as a *ſeal* or an *otter*."

Had Lord M—— been as converſant in anatomy as he has ſhewn himſelf in the philoſophy of mind, he certainly never would have advanced ſuch a poſition; for all naturaliſts agree, that in the amphibia in general, and particularly in the *ſeal* and *otter*, the *foramen ovale* never ſhuts up, by which means two thirds of the blood paſs, as in the fœtus *in utero*, from the right to the left ventricle without going through the pulmonary ſyſtem. I have indeed ſeen a ſingle inſtance of the *foramen ovale* open in the adult, but it is a rare occurrence, and beſides, were that always the caſe, ſtill reſpiration would be abſolutely neceſſary to man, on account of the noxious phlogiſticated matter, which is perpetually ſeparating from his blood.

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derable variations, without any immediate injury to their economy. Thus, if we plunge a frog into warm water, we ſhall find, on introducing a graduated thermometer into its ſtomach, that the temperature has riſen ſeveral degrees, without ſenſibly affecting the health of the animal. In worms, and thoſe fiſhes which have but one ventricle of the heart, and no lungs, the degree of heat is conſiderably leſs than in amphibious animals, and is ſtill more variable than in them. Dr. Martin has ſhewn that the former are capable of great viciſſitudes in that reſpect *.

SECT. II.

The Uniformity in the Temperature of Animals is remarkable.

THOUGH we have found a conſiderable latitude of temperature in the different claſſes of animals, there is a ſurpriſing uniformity in that, which is peculiar to each genus and ſpecies. The difference between the ſpecies of the ſame genus never exceeds a few degrees; and the diſtinction between the loweſt

* De Cal. Animal.

genus,

genus, and the highest, is not greater. Thus in the whole variety of the bird kind, the temperature is nearly one and the same; and as far as our experiments go, it is likewise steady in the amphibia. With respect to the more imperfect animals, they are too dependant on the influence of external circumstances to preserve uniformity of heat.

But one of the most remarkable properties of animal heat, is the uniformity which it is observed to maintain under the greatest irregularity of size. In all the variety of quadrupeds, birds, and cetaceous fishes *, we find nothing depending on that circumstance; for the thermometer rises to the same degree, whether applied to the mouse or the elephant, the wren or the ostridge, the sea-calf or the whale; and we cannot perceive that age, size, or temperament, produce any material difference. That this at least is the case with respect to man, *De Haen* has ren-

* Nature, on this tribe, hath bestowed an internal structure in all respects agreeing with that of quadrupeds. Cetaceous fish, like land animals, breathe by means of lungs, have the power of uttering sounds, are furnished with organs of generation, copulate, bring forth, and suckle their young.

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dered ſufficiently plain, by a courſe of accurate experiments on ſubjects of both ſexes, from the earlieſt infancy, to extreme old age *: And if we be allowed to reaſon from analogy, we muſt ſuppoſe that the ſame uniformity of temperature extends to all the more perfect tribes of animals.

Before the days of the immortal *Harvey*, it was the general opinion of phyſiologiſts, that certain parts of the body, in particular the thoracic viſcera, were of a temperature ſuperiour to the other parts of the animal; but it is now, I think, evinced by the moſt accurate obſervations, that in every individual, it is uniformly the ſame over the whole body ‡; and if any difference has at any time ſeemed to ſubſiſt in a healthy animal, between the external and internal parts, it is to be entirely attributed to the communication with the colder ſurrounding medium, and the difficulty of preventing the thermometer from being affected by it. With reſpect to thoſe parts, which are without the courſe of circulation, the cuticle, hair, nails, &c. they are always

* Ratio Medendi.

‡ Malpighii oper. poſth.

at a mean degree betwixt the heat of the animal and the ambient air.

SECT. III.

Connexion betwixt the State of Reſpiration, the Colour of the Blood, and the Degree of Heat in Animals.

FROM the view which has been taken of the heat of the different tribes of animals, it is clear that there ſubſiſts a ſtriking connexion betwixt the ſtate of reſpiration, and the degree of heat peculiar to each. This is ſo univerſally the caſe, that from the mammalia * and birds, down to thoſe animals which ſeem almoſt deſtitute of the organs of reſpiration, we find the gradations pretty regular of a deſcending ſcale of heat. Hence we learn that not only the degree of heat, and the ſtate of

* Under the claſs of mammalia, *Linnæus* has arranged thoſe animals whoſe heart has two auricles and two ventricles, with a warm red blood; which breathe by means of the lungs reciprocally; whoſe jaws are incumbent, covered, and furniſhed with teeth: the males of which have an external penis, the females a clitoris and nymphæ: are viviparous, and ſuckle, *&c.*

reſpiration are in due proportion to each other, but likewiſe that both are intimately connected with the more or leſs perfect ſtate of the animal. But how imperfect ſoever animals may be, the alternate inſpiration and expulſion of air ſeems ſo neceſſary to their exiſtence, that if they be not furniſhed with a pulmonary ſyſtem, they are with ſomething equivalent to it; for thoſe animals, which want both lungs and moveable ribs, and conſequently a dilatable cheſt, have that deficiency compenſated in ſome degree by an analagous mechaniſm. All fiſhes, for inſtance, except the cetaceous and exanguious, have branchiæ or gills, which do the office of lungs, receiving and expelling alternately the water; by which means the ſame change is produced upon the blood, as in the lungs of the more perfect animals *.

Inſects, in general, have no thorax or diſtinct cavity for the heart and air veſſels, but have the latter diſtributed through the whole trunk of the body, by which they communicate with the external air through ſeveral

* *Galen* was aware of the reſpiration of fiſhes by their gills, which he ſuppoſed every where perforated, and pervious to air and vapour, but not to water.

ſpiracles;

ſpiracles; and to theſe are faſtened little tracheæ, or wind-pipes, which ſend their branches to all the muſcles and internal organs, and ſeem to accompany the blood veſſels all over the body, juſt as the ramifications of the wind-pipe do in the lungs of the more perfect animals. By that wonderful piece of mechaniſm, the whole frame of thoſe little animals, is alternately inflated and compreſſed, and the ſame changes induced, as if they were furniſhed with lungs.

The only animal exempted from the neceſſity of breathing is the fœtus; but it, while included in the womb, ſeems to have little more than a vegetative life, and is by many conſidered as a real Zoophyte growing to the mother by the umbilical cord, as plants do to the earth by the ſtem; and to be ſure it ought rather to be accounted a Cion, or branch of the mother, than a diſtinct and ſeparate animal. With reſpect to the phyſical neceſſity that there is for reſpiration being more or leſs perfect, according to the higher or lower degree of heat peculiar to the animal, it will be more properly conſidered hereafter.

The difference of temperature in different claſſes of animals, is not leſs intimately con-

nected with the crasis and colour of the blood, than we have found it to be with the state of respiration. All the more perfect animals, man, birds, and quadrupeds, which are known to generate the highest degrees of heat, have likewise the greatest proportion of red globules in their blood. The amphibious kind, which are of an inferiour temperature, have a proportional diminution in the quantity of red blood. Next to these, come the branchial fishes *, which are still more deficient in both respects, and seemingly in an exact proportion; and lastly those, which are destitute of gills, have their fluids as transparent as the element in which they live, and are nearly of the same temperature. Of this cold exanguious tribe are many species of shell-fish, oisters, cockles, sepiæ, *&c.*

* Dr. *Haller*, in his *Elem. Physiol.* expresly says, that in the blood of fishes there is neither heat nor density, and but little crassamentum; and this assertion is supported by Lewenhoeck's microscopical experiments.

SECT.

SECT IV.

Connexion betwixt the State of Circulation and the Degree of Heat in Animals.

THE connexion, which ſubſiſts between the degree of heat, the ſtate of reſpiration, and the colour of the vital fluid, ſeems to have eſcaped the notice of moſt phyſiologiſts, both ancient and modern; but all, who have in the leaſt attended to the laws of the animal economy, agree in allowing a neceſſary connexion betwixt the degree of heat generated, and the ſtate of circulation; and every one muſt have obſerved, that, whilſt the motion of the circulating maſs continues vigorous and unimpaired, the temperature of the body ſuffers no change from the influence of external circumſtances; but no ſooner has the heart ceaſed to play, and the blood begun to ſtagnate in its canals, than the abſence of the generating cauſe of heat becomes manifeſt: for the now exanimated maſs ſinks to the temperature of the bodies around it.

On whatever principles the production of animal heat has been explained, no theoriſt,

as

as far as I know, has queſtioned its relation to the ſtate of circulation. We find indeed in the annals of phyſic, a few ſeeming deviations from the common procedure of nature; but they are both too few, and too ambiguous, to affect in any degree the general fact.

Mr. *De Haen* *, who has attended in a particular manner to the ſubject of animal heat, brings, as unanſwerable objections to its mechanical generation, two caſes, which fell within his own obſervation. In the one, he found that the temperature of his patient, which, during the courſe of an inflammatory fever, had never riſen above 103 degrees, ſtood at the time he expired, and for two minutes after, at 106°. From the other it appears, that the heat of a perſon, who was dying of a lingering diſtemper, roſe in the laſt agony from 100 to 101°, and continued there ſtationary for two hours; and even at the expiration of fifteen hours, had only fallen to 85°, though the ambient medium did not exceed 60°.

I am entirely of *De Haen*'s opinion, that thoſe caſes are utterly inexplicable on mecha-

* Ratio Medendi, *vol.* I.-II.

nical

nical principles; but it will, I hope, be ſound, that they admit of a ready ſolution, on the theory to be hereafter propoſed. In the mean time, it may not be improper to obſerve, that the vital principle is not always immediately extinguiſhed on the ceaſing of reſpiration. Breath and life have in all ages been conſidered in breathing animals, as ſynonimous; a notion, which has ever been productive of the moſt unhappy conſequences to mankind, and daily, I am bold to ſay, ſends thouſands to the grave before their time. For we cannot, I think, doubt, when we reflect on the many fortunate recoveries of perſons drowned, and apparently dead, that the living principle may remain in the body after reſpiration, and all the other vital and natural functions ſeem at an end, and be again by proper means excited into action *.

It may therefore reaſonably be ſuppoſed, that though in the two caſes mentioned by *De Haen*, the organs of reſpiration had ceaſed to act, yet not only the principle of life was ſtill preſent for ſome time, but that

* It appears from the records of the Humane Society, that, within this twelvemonth, there have been 250 drowned perſons brought to life in Britain.

even

even ſome degree of circulation was carried on, and conſequently ſome heat generated. Now, if the final cauſe of reſpiration really be, as I hope afterwards to ſhew, to carry off the phlogiſton extricated in the courſe of circulation, and to temper the heat generated in the ſyſtem, it plainly follows, that if any heat be ſtill produced after this function ceaſes, that heat muſt accumulate in the body, and of courſe raiſe, or at leaſt keep up its temperature. This, however, is a thing which will ſeldom obtain, as the ſame atony and debility that put a ſtop to the action of the organs of reſpiration, muſt, in a ſhort time, entirely aboliſh the propelling power of the heart and arteries.

It is, however, highly probable that the circulation is not ſtopt ſo early as is generally imagined. There are two kinds of motion peculiar to the heart and arterial ſyſtem; the one is by anatomiſts termed *ſyſtaltic motion*, and conſiſts ſimply of an alternate contraction and dilatation; the other they ſtile *ſubſultory motion*, as cauſing the beating of the heart, and contributing to the pulſation of the arteries.

This ſubſultory motion is ſynchronous with the ſyſtaltic, and unleſs the ſtrength be nearly exhauſted,

exhausted, is constantly performed; but when the vital powers are much debilitated and sunk, then it becomes imperceptible, and there is nothing but the systaltic motion, which however is sufficient to empty the ventricles of the heart, and carry on life for some time.

M. *de Lamure* * has, I think, very clearly demonstrated, that the pulsation of the arteries does not depend, as is generally taught and believed, on their simple dilatation from the sudden increase of lateral pressure against their yielding elastic coats, produced by the successive columns of blood which are forced into them by the *vis a tergo*, but that the subsultory motion is communicated by the heart to the whole arterial system, by which a displacing, or locomotion, of the whole canal is super-induced; and indeed we can hardly doubt, that to that circumstance is chiefly owing the phænomenon of arterial pulsation; for so exactly does it correspond with the beating of the heart, that when that center of motion acts with strength and vigour, the pulsation of the artery is strong; but when it flags, and the vital powers are brought

* Mem. de l'Acad. des Sciences 1765.

very low, the blood glides languidly along, and there is as little pulſation in the arterial, as in the venous ſyſtem.

This account of the motion of the heart and arteries throws no inconſiderable light on the caſe of perſons, who, after remaining for ſome time to all appearance dead, have again revived; for as it is highly improbable that life ſhould ſubſiſt without ſome degree of circulation, it is abſurd to ſuppoſe that, after an entire ceſſation of motion in the ſanguiferous ſyſtem, and the well-known changes in the vital fluid conſequent on ſtagnation, the circulation ſhould be again reſtored. In the caſes therefore of fainting and drowning, it is to be preſumed that the blood is carried through the circulatory veſſels by the ſyſtaltic motion alone, and that the ſubſultory is, for the time, entirely ſuſpended.

Is it not our duty then to be extremely cautious in pronouncing people irrecoverably loſt, and interring them before evident ſymptoms of putrefaction have taken place; ſince it appears that life may remain after the ſubſultory motion of the heart hath ceaſed, the pulſation of the arteries become imperceptible, and every indication of breathing diſappeared. If the chief and ultimate uſe of reſpiration

ſpiration in the animal economy be, as has been already hinted, to carry off the phlogiſton extricated from the blood in the courſe of circulation, it is obvious, that when the action of the heart and arteries is very weak and languid, little or no phlogiſton will be ſeparated, and therefore a conſtant renewal of air will, for the time, become in a great meaſure unneceſſary: And we are confirmed in thinking ſo, by obſerving that thoſe animals, whoſe circulation is moſt ſluggiſh, and temperature low, breathe of all others the ſloweſt, and can, for a given time, with the leaſt inconvenience, deſiſt from breathing.

Upon the whole, I am inclined to think, that though ſome deviations may have occurred from the ſtrict connexion, which is known to ſubſiſt between the ſtate of circulation and the degree of heat in animals, they are by no means ſo frequent as phyſicians, for want of accuracy in their thermometrical obſervations have imagined. *Sydenham* recites ſeveral hyſterical caſes, in ſome of which a ſtrong full pulſe was attended with an unuſual degree of cold, whilſt in others, a ſmall and languid pulſe was accompanied with a preternatural heat: But as it does not appear from the context, that he had meaſured thoſe

different degrees of heat by the thermometer, it is natural to ſuppoſe that he had entirely relied on the feelings of the patient, which, it is well known, are in this reſpect extremely fallacious: For it is not uncommon to hear a patient complain of an intolerable ſenſation of cold, while the temperature of the body is actually ſeveral degrees above the natural ſtandard.

It will however be in general found, that an increaſed action of the ſanguiferous ſyſtem is attended with a proportionably increaſed degree of heat; and a languid or interrupted circulation, with a diminution of the natural temperature. Thus in all topical inflammations, where the velocity of the blood is conſiderably augmented, on applying the thermometer, it uniformly indicates an increaſe of heat in the part; and on the contrary, when the circulation is partially interrupted in any organ, or member of the body, there, a very ſenſible diminution of the uſual heat will be obſerved; and if a gangrene has ſupervened, none at all will be generated.

Such is the hiſtory of animal heat; and in this, as in all phyſiological queſtions, an accurate account of the phænomena is the only baſis, on which convictive reaſonings, ſatisfactory

ſatisfactory explanations, and ſound theories, can be eſtabliſhed. We ſhall therefore avail ourſelves of the conſideration of the foregoing facts to eſtimate the degree of credit, which ought to be given to the hypotheſes hitherto advanced by phyſiologiſts on the ſubject.

CHAP.

CHAP. III.

A View of the prevailing Opinions on the Cause of Animal Heat.

AS there is perhaps no ſubject in phyſiology, and very few in philoſophy in general, that has more engaged the attention of the ingenious than that of animal heat, ſo no one has given birth to a greater number of hypotheſes.

Philoſophy is the proper object of the intellect, and theorizing, if I may be allowed the expreſſion, the favourite proceſs of the human mind; but, as we are able only to perceive a few links of the chain of cauſes and effects, by which the various phænomena of nature are joined together, we are frequently miſled in our inveſtigations, by attributing too great an influence to thoſe powers which preſent themſelves moſt obviouſly to our ſenſes; and therefore theories, according to the different points of view, in which they ſtrike the fancy, are alternately received and rejected.

Nothing

Nothing, however, in my opinion, has been so eminently detrimental to the progress of science, as a blind and servile deference to authorities; for how many even palpable absurdities have, through a long series of years, passed unchallenged, under the venerable sanction of some great name; but to the honour of the present age, that bar to the advancement of knowledge is, in a great measure, removed from the schools of science; and that truly philosophic scepticism, which is always attended by a free and active spirit of enquiry seems now to actuate the minds of men in all literary pursuits. It is with particular satisfaction that we observe, that, in all physical and medical disquisitions, men in general have learned to appeal to common sense and experience, and to regard the *ipse dixit* of no man farther than his doctrine is countenanced by facts, and supported by sound reasoning.

To enter into a minute detail of all the various opinions, which have been offered on the cause of animal heat, would far exceed the limits of the present plan; and therefore both on this account, and because most of them may be referred to one or other of the three general causes of heat, MIXTURE, FERMENTATION,

MENTATION, and MECHANICAL MEANS, we ſhall conſider them under thoſe three heads; and by ſo doing, we ſhall not only be enabled to bring them into a more narrow compaſs, and perhaps a clearer point of view, but ſhall likewiſe avoid many uſeleſs repetitions, and tedious quotations.

SECT. I.

Of CHYMICAL MIXTURE.

WHEN Chymical Phyloſophy came into vogue, and prevailed in the theory, as well as the practice of medicine, almoſt every operation in the animal machine was attributed to the effect of ferment, or mixture. We need not be at all ſurprized that men, who never extended their views beyond the bounds of their laboratory, nor ever contemplated the other parts of nature, became poſſeſſed of the idea of the univerſal application of thoſe energies, which they had perceived in ſuch a variety of inſtances; and that imagining their influence unbounded, they attempted to explain even the functions peculiar to life, on principles deduced from their

their acquaintance with that one ſet of cauſes.

Thus, from obſerving that on mixing certain bodies far below the temperature of the human body, a degree of heat ſometimes riſing to actual inflammation was produced, they, without further inveſtigation, pronounced mixture the ſole cauſe of animal heat. Various, however, were the opinions not only reſpecting the place where the mixture happened, but alſo concerning the nature of the fluids of which it conſiſted.

Vanhelmont, *Sylvius*, and ſeveral other chymico-phyſiologiſts, ſuppoſed that the mixture took place in the inteſtinal tube, and aſcribed it to an efferveſcence betwixt the pancreatic juice and the bile. Others diſcovered acids in one place, and alkalis in another; but the general opinion for near two centuries was, that aceſcent fluids taken in, meeting with alkaline already prepared in different parts of the body, gave riſe to the degree of heat peculiar to animals. But all who are in the leaſt converſant with the laws of the animal economy, need not be told that theſe opinions are mere conjectures, founded on facts gratuitouſly aſſumed. No experiments have ſhewn either an aceſcency or alkaleſcency in

the bile, that is ſufficient to unite with the other animal juices, and generate the heat of animals. But to avoid much diſcuſſion, did we even admit the ſuppoſition in its full extent, ſtill it would be found by no means ſufficient to account for the ſtability of animal heat in different climates and ſeaſons; its equability all over the body when in health; its partial increaſe in topical inflammations, or hardly indeed for any one phenomenon attending its production.

Since then it appears that the fluids ſuppoſed to be mixed, the place in which the mixture is made, and every other circumſtance pertaining to it, are equally ill aſcertained, and ſeconded by analogy, none will, we preſume, heſitate to reject every hypotheſis of the cauſe of animal heat, founded on the effects of mixture.

SECT.

SECT. II.

Of FERMENTATION.

WHEN a more accurate and extenſive knowledge of the various operations of nature had convinced phyſiologiſts of the abſurdity of explaining the vital functions of animals, and the ſeveral changes, which take place in the living body, by the effects of chymical mixture, moſt opinions, which had ſprung from that ſource, were exploded from the ſchools of medicine, and amongſt others that, which ſuppoſed animal heat to ariſe from the effervеſcence of fluids of oppoſite natures taken into the ſtomach, or generated in the economy. In its ſtead, fermentation was ſubſtituted. All had obſerved that fermentation was generally accompanied by the production of heat, and few were ignorant that, that identical proceſs, or one extremely analogous to it, was conſtantly going forward in living animals; and it was not without ſome appearance of truth, that phyſiologiſts attributed animal heat to that cauſe.

Formerly there were various modifications of this opinion; but of late it has been chiefly confined to one ſpecies of fermentation, the putrefactive, which to be ſure is more conſentaneous to experience and ſound philoſophy. For although animal ſubſtances be either directly or indirectly produced from vegetables, as all animals live on vegetables, or on animals that have lived on them; and though they may be ultimately reſolved into the ſame principles *, yet they are certainly combined in a different manner; for they conſtitute compounds, the nature of which are eſſentially different; and of the three ſtages of fermentation, the vinous, acetous, and putrid, the laſt is the only one, to which they ſhow a tendency. Milk, indeed, tends to the acetous, and even to the vinous fermentation; but as it can hardly be conſidered as perfectly animalized, it ought not to be reckoned an exception to the general poſition:

* When animal matter is ſubjected to chymical analyſis, it is ultimately reſolved into water, earth, air, and phlogiſton. As to the ſaline matter, it plainly owes its origin to a combination of phlogiſton with earth, or ſome of the other conſtituents. The reſolution of vegetable ſubſtances is in no ſhape eſſentially different from that of animal matter.

tion: And though it be readily admitted that animal matter is extremely apt to putrify, and that even in the living body there is a constant tendency to that process, yet it may, I think, be shewn, that the degree to which it takes place, can have little or no share in generating the heat of animals. In the first place, the effect of any degree of putrefaction in producing heat, is to this day so ill ascertained, that with many ingenious philosophers it is absolutely a problem, whether or not animal substances, during the putrefactive process do ever generate heat; and many circumstances tend to show that their doubts are not without foundation. Neither M. *Beaumé* *, nor Doctor *Pearson* ‡, who made several accurate experiments, with a view to ascertain this disputed point, could, by the assistance of the most sensible thermometers, discover the least difference betwixt the temperature of the putrefying mixtures, and the surrounding medium; and were the putrefaction of animal substances really attended by the generation of heat, we should

* Mem de l'Acad. des Sciences.

‡ Dissert. inaug. de Putred. *Edin.* 1773.

expect

expect to find it greater in proportion to the bulk of the putrefying mass. That however is not the case, for it has been often found that the largest masses of animal matter, such as the carcass of a large whale laid out and exposed to the air in so putrid a condition, as to affect all the neighbourhood with an intolerable stench, did not to the persons handling it feel sensibly hotter than the circumambient air.

It may therefore, I apprehend, be concluded, that if any degree of heat be really generated by the putrefaction of animal substances, it is extremely inconsiderable. This inference is not only supported by the facts above-mentioned, but in some measure countenanced by animal matter not going through the vinous and acetous stages of fermentation, which most vegetables do before they reach the putrefactive; and though it be readily admitted, that the fermentation of vegetables is accompanied by the generation of heat, yet I cannot agree with the general opinion of naturalists, that it is the effect of the putrefactive stage. It is well known to farmers that if a mass of green hay be put up when very moist, such a degree of heat will be produced, as even to break out into ac-

tual

tual flame; and that circumſtance has been univerſally attributed to the proceſs of putrefaction going on in the maſs; but from my own obſervation, and the moſt accurate accounts that I have been able to collect reſpecting that fact, it appears to me ſufficiently manifeſt, that it is only while the vegetable is undergoing the vinous and acetous ſtages of fermentation, that any heat is generated; and that as the putrefactive advances, the heat gradually decreaſes.

This is further illuſtrated and confirmed by the phænomena attending the proceſs of making malt. I would not however poſitively aſſert that there are no exceptions to this general propoſition; but I am perſuaded they are few; at leaſt, I have not yet heard of any that were not extremely ambiguous, or which might not be accounted for on different principles.

Now, as it pretty evidently appears that heat only accompanies the two firſt ſtages of fermentation, and that animal ſubſtances are incapable of undergoing either of theſe, it is not without reaſon, that we queſtion whether the putrefaction of ſuch ſubſtances does, on any occaſion whatever, excite heat. We do indeed in ſome writers meet with accounts

of

of heat produced by putrefying heaps of hoofs, horns, and hides of animals; but there is, I think, reason to suspect that on those occasions there have been some other matters present, capable of undergoing the vinous and acetous fermentations. Be that, however, as it will, we have no just grounds for ascribing the generation of animal heat to the effects of putrefaction. No experimentalist, as far as I know, has ever attempted to show that in the precise degree towards putrefaction, which takes place in living animals, any heat is produced; nor does it appear from facts and experiments, that there is an increase of heat corresponding to the increase of putrefaction. In the most putrid state of the fluids, as in the sea-scurvy, the temperature of the body is seldom observed to rise above the natural standard; and in the case of a topical mortification, instead of an increase, there constantly obtains a diminution of heat; for the gangrenous part always sinks to a mean degree between the temperature of the body, and the circumambient air.

It was formerly mentioned that the heat of animals is closely connected with the motion of the sanguiferous system; but it is known

that

that motion, inſtead of promoting the putreſcent tendency of the fluids, is one of the moſt powerful means employed by nature to check its too rapid progreſs, and obviate the deleterious conſequences, that would inevitably ariſe to the economy from it *. Some phyſiologiſts from obſerving that putrid and peſtilential diſtempers are generally accompanied with a quickened circulation, and an unuſual degree of heat, have not only attributed the heat, but likewiſe the preternatural tendency to putrefaction, to the accelerated motion of the blood; but as heat, on every other occaſion, is certainly known to be, of all ſecondary cauſes, the chief promoter, both in animal and vegetable ſubſtances, of the putrefactive fermentation, it is more conſiſtent with juſt reaſoning and analogy, to conſider the increaſe of heat in putrid fevers rather as a cauſe, than an effect of the putrefaction going forward in the body.

But an argument, which at once overturns every thing that can be advanced in favour of

* "Conſtat certa et firma experientia humida quiete ad putredinem maxime diſponi, *motu* vero continuo, tam inteſtino quam locali, ab ea libera permanere."——*Hoffman. Med. Rad. Syſt.*

the generation of animal heat on the principles of putrefaction, is, that heat is far more considerable in a living, than in a dead body *; and no rational physiologist will deny that the putrid fermentation is going forward more rapidly in the latter, than in the former. From these few of the many arguments, which might have been brought against ascribing the heat of animals to the effect of mixture or fermentation, we have the justest grounds for deserting both hypotheses, and shall therefore, without dwelling longer upon them, proceed to our third general head of opinions.

* "Si a putredine calor nasceretur cadaver caleret post mortem, et febri torqueretur ardentius quam dum viveret." *Helmont. de Feb.* Cap. I.

SECT.

SECT. III.

The MECHANICAL GENERATION *of* ANIMAL HEAT.

WHEN we explained the phænomena of animal heat, we had occasion to observe that there subsists a very intimate and striking relation betwixt it, and the state of circulation; and we then shewed that, whilst the action of the heart and arteries continues unimpaired, the natural temperature continues stable and unchanged; but that, according as the circulation is more or less vigorous, the heat of the body is encreased or diminished; and that, when a final stop is put to the action of the heart and arteries, it is influenced by the surrounding medium, in the same manner as the most unorganized mass of matter.

From this view of things, physiologists have not only been led to consider the circulation of the blood and the temperature of the body as very closely connected with each other, but have even attributed the generation of heat in animals to the mere mecha-

nical effects of motion. This has been the favourite opinion of phyficians ever fince the complete difcovery of the circulation by the celebrated *Harvey*, and is no doubt countenanced by many fpecious arguments. So numerous and ftriking are the facts, which evince the connection betwixt the ftate of motion in the fanguiferous fyftem, and the temperature of the body, that no doubt can be juftly entertained of the latter being, in a fecondary manner, at leaft, the effect of the former; but on what principles the fact is to be explained, we are now to enquire.

All phyfiologifts, as far as I know, admit, that the power of generating heat is not perfect until the circulation of the blood be fully eftablifhed all over the body. The circumftances which attend the incubation of eggs, fet this matter in the cleareft light; fince it is now found that artificial warmth, in whatever manner it be excited, anfwers equally with that of the parent bird in hatching fecundated eggs, it is apparent that her brooding over them can have no other effect, than to fupport that genial degree of heat, which

is neceſſary to the developement of the chick *.

The continual application of heat at the beginning of incubation, and during the greater part of the proceſs, is ſo abſolutely neceſſary, that if it be in a cold ſeaſon withdrawn only for a ſhort time, the eggs always acquire the temperature of the ſurrounding medium; and if the rudiments of the chick have already received the principle of life, it never fails to be irretrievably loſt; but, at a more advanced period, when all the parts of the animal are completely formed, and the circulation fully eſtabliſhed, the dam may relinquiſh the neſt, for a longer time than would be ſafe for the chick, did all its heat depend upon her; nay, ſhe may even, at that period, ſtay from it without the heat of the eggs being diminiſhed in any material manner, long enough for eggs, which have within them no principle of heat, to ſink to the temperature of the circumambient air.

* The art of hatching chickens, by means of ovens, has been long practiſed in *Egypt*, and has of late been reduced to certain principles by the ingenious M. *Reaumur*. The degree of heat neceſſary is nearly that of the human body.

This

This fact may be explained in the subsequent manner:—The most accurate microscopical observations show, that in the embrio of every species of animal, the action of the heart takes place very early; but the circulation, it is obvious, can only extend very gradually, and in proportion to the progressive evolution of the rudiments, and the more complete formation of the parts. Hence, it of course follows, that as the generation of heat in animals is connected with the state of motion in the vascular system, that must be imperfect, till this be fully established; and that is the reason why the chick *in ovo* generates very little heat till towards the end of incubation, when, being complete in all its parts, it generates heat sufficient to enable it to support, with impunity, a degree of cold, which, at an earlier period, would inevitably have proved fatal.

These facts tend to shew that no animal whatever, before it be perfect in all its parts and functions generates heat enough to support life; and that the circulation hath no sooner taken place, than the temperature falls, or rises, according as various causes diminish, or increase, the motion of the blood. Of this we have the clearest evidence in the

case

caſe of dying perſons. When the powers of life are brought very low, and vigour is wanted to carry on the uſual functions, the ſanguiferous ſyſtem becomes affected in the moſt ſenſible manner, and its action being no longer able to propel the blood to thoſe parts of the machine which are moſt diſtant from the heart, their temperature begins to ſink, and continues to diminiſh in proportion as the circulation declines; and hence it is uſual to judge of the approach of death by the coldneſs of the extremities; for, notwithſtanding one or two ſeeming exceptions, it is an incontrovertible truth, that exanimated bodies loſe heat in a given temperature of air, as faſt as any unorganized maſſes of matter, of the ſame bulk, of a texture any way ſimilar, and heated to the ſame degree.

But notwithſtanding every thing that phyſiologiſts have advanced, with reſpect to the connexion between the action of the circulatory ſyſtem, and the heat of animals, they have, I am afraid, made but a very inconſiderable progreſs towards the ſolution of the problem, as the main difficulty reſts on explaining how the circulation can effect the generation of heat. This queſtion, to thoſe phyſiologiſts, who are perſuaded that every phænomenon

phænomenon of the animal economy is explicable on mechanical principles, has always appeared extremely simple, and almost capable of mathematical demonstration; for, as the highest degrees of heat are generable in certain bodies in consequence of friction, and as the human body is a machine constructed on the most exact mechanical principles, the production of heat seemed no more than a necessary effect of the motion of the circulating mass; and accordingly two different hypotheses were confidently advanced, and have now, for a century, prevailed in the schools of physic.

The first, supposes animal heat to depend on the reciprocal action of the fluids upon the solids; that is, of the blood upon the internal surface of the containing vessels: and this is still the more general opinion.

The second hypothesis considers the heat of animals as excited by the continual agitation and unremitting intestine motion of the insensible particles of the blood upon one another. How far both, or either of those opinions, are in fact established, we are now to inquire; and first, of the mutual action of the fluids and solids.

There

There are many arguments deducible even from the known laws of mechanics, that tend to invalidate the ſuppoſition which attributes the heat of animals to the attrition of the blood againſt the ſides of the canals through which it glides. In the firſt place, it will, I ſuſpect, be extremely difficult, not to ſay impoſſible, to bring a ſingle ſatisfactory inſtance, in which heat is generated in conſequence of ſolid and fluid bodies acting upon each other. Two inſtances however are inſiſted upon by the favourers of the above hypotheſis; the one is, that bodies moving rapidly through the air acquire heat; for a cannon-bullet is found hot after paſſing through a long tract of air. The other is, that quick-ſilver ſtrongly agitated in a glaſs phial generates heat.

Thoſe facts, how ſtrong ſoever they at firſt appear, will be found on diſcuſſion to have very little weight. But ſo ardently do we deſire to find every phenomenon in nature analogous to ſome other, with which we are already acquainted, and explicable on the ſame principles, that we often fancy reſemblances between things, where in truth there are none, or none of any conſequence. Analogical arguments preſent themſelves readily to a

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warm imagination, whilst those more decisive ones, arising from experiment, can only be obtained by labour and painful attention. Hence, even in the most important subjects, the former are substituted in the place of the latter, and after the most beautiful system has been formed, some paltry little fact is discovered, which overthrows the whole, and turns its fabricator into ridicule.

The first instance given, in support of the abovementioned opinion, is futile; for though a cannon bullet be found hot after passing through a long tract of air, yet it doth not absolutely follow, that the heat acquired is the effect of the motion; on the contrary, there is reason to suspect a deception in the case; for it is certainly much more probable that the heat found in the ball is excited by the explosion of the gun-powder, and the violent attrition betwixt it and the rough sides of the cannon, than by its gentle friction on the yielding air. No certain conclusions at least can be drawn, nor any stress laid upon the fact, till such time as, by intercepting the ball at the mouth of the cannon, we have ascertained the degree of heat with which it is then fraught.

But

But admitting the fact, in the very manner in which the mechanical physiologists represent it, still I maintain that nothing can be deduced with respect to the generation of heat in animals. In no one point of view is the analogy just. Where is the comparison between the elasticity of the air and that of the blood? Between the solid hardness of the bullet, and the soft texture of the sanguiferous canals? In a word, what approaching point, or comparison of velocity is there between the circulation of the blood and the rapid flight of a cannon-ball through the air? It may therefore be presumed, that in sound reasoning, all arguments deduced from that source will weigh extremely little.

Let us now enquire into the merits of the second instance, the generation of heat by the conquassation of quick-silver. It is asserted, and with some foundation, that a certain degree of heat may be produced by a strong and long-continued agitation of quick-silver in a hard vessel; but is not the difference between the density of the particles of the mercury, and the fluidity of those of the blood manifestly too great to admit of any analogy? However, we have no need to rest our objections on that ground, for it is an

incontrovertible fact, that no heat is generated in that way, unless a portion of the mercury be previously transmuted into a dry powder: and hence it is allowable to infer, that the heat produced is the effect of the friction of this powder against the sides of the containing vessel.

Various other arguments more immediately arising from the obvious structure of the animal machine discountenance the mechanical production of its heat. The effect of attrition, according to the common laws of mechanicks, is known to be in proportion to the roughness of the surfaces; for the impulses must necessarily be in an exact ratio with the prominences, that is the resistance; but the fluids of animals, and the inside of their circulatory canals, being as smooth as we can possibly imagine any thing to be, what shadow of probability is there of animal heat being generated in that way? Besides, it does not even escape the rudest mechanick, that the heat, which rough surfaces by rubbing against each other should produce, may be effectually prevented by the interposition of fluid bodies; which, being supposed spherical, may be accounted so many friction wheels: at least, by filling up the inequalities,

ties, they make the ſurfaces ſmooth, and obviate the mechanical production of heat. The caſe is ſtrictly ſimilar, with reſpe to the circulating fluids of animals which are not even in contact with dry ſolids; for from the internal ſurface of the veſſels is perpetually exuding a mucuous liquor, with which they of conſequence are always lubricated: and hence the movement of the blood thro' its canals is properly no more than the action of fluid upon fluid.

In every point of view, in which we have contemplated the mechanical generation of animal heat, we have found it involved in doubts and difficulties not to be reſolved; and yet many hypotheſes, equally doubtful in themſelves, have been reared on the ſame infirm baſis. Several phyſiologiſts aſſuming it as a fact, that the heat of animals is generated by mechanical means, were only ſolicitous to explain ſome of its principal phænomena, and in particular its equality all over the body; but though they ſeem all to have believed that the heat of animals is produced only in one part of the living ſyſtem, and ſupported in the reſt by communication, hardly two of them, of any eminence, have agreed with reſpect to the particular organs

or

or place, in which that ſingular effect is brought about.

Dr. Douglaſs and his followers maintain, that heat is ſolely generated in the ſmaller capillary veſſels, becauſe, ſays he, ſingle globules only can paſs thro' thoſe at one and the ſame time, and therefore muſt be applied with larger ſurfaces. We are likewiſe told, that in thoſe veſſels the fluids are reduced to ſuch ſmall particles, that the attrition may be conſidered as that of ſolids acting upon ſolids; but we can perceive no foundation for accounting one of thoſe red globules a ſolid body: for, granting that diviſion to be the minuteſt poſſible, thoſe particles are by no means atoms, but are ſtill maſſes of fluid, and of ſo ſoft and flexible a texture too, that they are univerſally allowed to be capable of elongation. In no one point therefore is this hypotheſis ſupported by facts, or analogy.

Another ſet of phyſiologiſts, from contemplating the ſlowneſs of the circulation in the capillary veſſels, maintain that animal heat is generated in thoſe parts of the machine, where the velocity of the blood is moſt conſiderable, and therefore in the lungs, and in that organ only. There is, I muſt confeſs, ſome foundation for fancying the

lungs

lungs the source, or elaboratory of animal heat, since in them the circulation is unquestionably more rapid than in any other part of the vital frame; for it is a matter of demonstration, that if the same quantity of blood is transmitted through the lungs, that circulates through the other parts of the body in the same period of time, which is indisputably the case, the velocity must be as much greater, as the capacity of the pulmonary is less than the capacity of the aortick system. But, as it is certainly known that the quantity of blood thrown out from each ventricle of the heart is nearly the same, and that the systaltic motion of each is equally frequent, and that the sum of the branches of the pulmonary artery, as well as of the aorta, is greater than that of the trunks, the difference in velocity is by no means so considerable as most physiologists seem to imagine. Be that however as it will, still the hypothesis is obnoxious to all the general objections against the production of heat by the attrition of fluids upon solids; and, in fine, to whatever has been urged against the effects of mechanical means.

But, though I reject every supposition of a particular seat or *officina* for the generation of the

the heat of animals, and consider the generating cause, whatever it be, as diffused over the whole machine, yet it is by no means necessary that it should be precisely equal in every part; for allowing it to be greatest in the largest vessels, from their equal distibution, and the rapidity of the motion of the blood, we may reasonably suppose, that as all bodies, which have received heat, require a certain time to lose it by communication, so the vital fluid will carry the heat generated in the large canals to the most distant capillaries. But, from an attentive consideration of the phænomena of topical inflammation, to me, the action of the smaller vessels seems much more considerable, than is generally taught and believed.

I am far, however, from agreeing with the ingenious Dr. *Martin*, who has taken a great deal of pains to show, that if animal heat, as he maintains, depends upon the attrition of the blood upon the sides of the containing vessels, it necessarily must be uniformly the same in every part of the body *. His manner of reasoning on the subject is extremely fallacious. He insists, that if fluids be pro-

* Edinb. Med. Essays, *Vol.* III.

pelled

pelled with celerities proportional to the diameters of their containing canals, the heat of theſe fluids, generated by attrition, will be equal; and therefore, that as in the animal machine, the internal ſurface of the ramifications of the arterial ſyſtem is increaſed in the ſame ratio as the velocity is diminiſhed, the generation of heat muſt be equal in every part of the ſame individual. But beſides that his meaſurement and calcul are by no means accurate, the principles, on which he goes, are altogether inconſiſtent with the common laws of mechanics: for, who ever attempted to demonſtrate that increaſe of ſurface compenſates for want of velocity? On the contrary it is well known, that if the velocity be diminiſhed in the inverſe ratio of the ſurfaces, that is, if the motion be diminiſhed as much as the ſurfaces are increaſed, the heat formerly excited will become on every occaſion proportionably leſs, and on moſt none at all will be generated.

Having ſhewn that the friction of the blood upon its canals will, in no ſhape, account for the production of animal heat, we ſhall briefly take notice of the opinion, which attributes it to the action of the ſolids upon one another.

The heart and arteries are almoſt the only organs which can be ſaid to be in perpetual motion; there being little or no motion in the muſcular ſyſtem, while the animal is aſleep: but granting every muſcle in the body to be conſtantly in action, ſtill not one probable argument could be deduced in favour of this hypotheſis.

In the firſt place, the moving parts, though ſtyled ſolid, are neither in their nature hard nor dry; two conditions abſolutely neceſſary to the mechanical production of heat.

In the ſecond place, no movements in the muſcular or vaſcular ſyſtem are either rapid enough, or carried on with a momentum ſufficient to excite heat in the coarſe way of attrition; and laſtly, it is apparent, from the moſt accurate microſcopical obſervations, that the minuteſt viſible ſtamen of every moveable fibre is lubricated with an unctuous mucilaginous liquor, evidently intended by nature to obviate the mechanical effects of friction. It is therefore, I apprehend, to be concluded from theſe conſiderations, that this theory of animal heat is by no means adequate to the ſolution of the queſtion, and may of

conſequence

consequence be dismissed without any further inquiry *.

The only hypothesis which now remains to be discussed, respecting the mechanical generation of the heat of animals, is that, which ascribes it to the intestine motion, or friction of the insensible particles of the blood on one another. This opinion, according to the manner in which it has been explained by physiologists, is liable to every objection, which has been urged against the production of heat by fluid acting upon fluid. Instead, therefore, of stopping to enumerate an infinitude of arguments, which evince the improbability of animal heat being generated in this way, I shall content myself with reciting a few lines from the learned *Schelhammerus*, who expresses himself on the subject with

* I am happy in this public opportunity of rectifying a mistake into which I had inadvertently fallen with respect to Dr. *Monro*. By trusting to inaccurate transcripts of his anatomical prelections, I published in my inaugural dissertation the above opinion, as that which was believed and taught by him. Having since however been informed that he publicly disclaims it, I no longer give it as his, and sincerely regret having attributed so unphilosophical an hypothesis to a physiologist so justly celebrated for extensive knowledge in every department of natural science.

no leſs good ſenſe than energy. "Provoca "(ſays he) ad experientiam totius naturæ "rerum. Moveant mihi et conquaſſent quemcunque humorem, et fluctus in ſimpulo "concitent integros dies noctesque. Æſtuet "mare, fluvii rapidiſſimi per immenſas rupes "provolvantur integra ſæcula, ne tepeſcent "quidem unquam, multo minus calebunt. "Numne igitur vident aliud quam motum "eſſe a quo caleat ſanguis? Etſi motu calor "ille augeatur * ?"

But though no phyſiologiſt has been able to demonſtrate, that the ſimple conquaſſation of fluids can raiſe any degree of heat by the means of mechanical attrition, yet it will not, it is hoped, be found very difficult to ſhow, from a variety of facts ſeconded by analogy, that the action of the ſanguiferous ſyſtem ſuperinduces thoſe changes in the common maſs of blood, which give riſe to the heat of living animals.

Before I proceed to explain how and on what principles that effect is produced, I cannot omit taking notice of the opinions, on this queſtion, of Dr. *Cullen* and Dr. *Black*, whoſe diſtinguiſhed merit in the phyſical

* De genuina febres curandi methodo.

world,

world, will hand down their names to the lateſt poſterity.

SECT. IV.

Dr. CULLEN'*s Doctrine of Animal Heat.*

DR. CULLEN, finding every explanation that had been offered on the cauſe of animal heat clogged with inſurmountable difficulties, was induced to attempt a ſolution of the queſtion on a new ſet of principles; but attentive to the candour and diffidence with which novel opinions ought to be broached, he delivers his, as little more than a mere conjecture. "May it not (ſays he) be ſuppoſed, that there is ſome circumſtance in "the vital principle of animals, which is in "common to thoſe of the ſame claſs, and of "like economy; and which determines the "effect of motion upon the vital principle, "to be the ſame, though the motion acting "upon it may be in different circumſtances *." The Doctor was driven to this ſuppoſition from the difficulty he found to

* Inſtitutions of Medicine, *p.* 224.

explain how ſo many animals, of a different age, ſize and temperament, ſhould poſſeſs very nearly the ſame degree of heat; and in which it is impoſſible to ſhow that the motion of the blood, in all its circumſtances, is alſo exactly the ſame, or that in the different animals in which the degree of heat is conſiderably different, the motion of the circulating maſs is in each correſpondent to the difference of temperature. Thoſe, I confeſs, are difficulties not to be reſolved on any theory hitherto communicated to the public: but ſome principles, it is hoped, will be laid down in the preſent treatiſe, on which they may, with no inconſiderable degree of probability, be explained.

In the mean time, it ſhall only be remarked, that granting the degree of heat not always to obtain in an exact ratio with the motion of the blood, and that this is an inſuperable objection to its mechanical generation, yet there appear no plauſible grounds for fancying that the effect of motion on the vital principle may be the ſame, while the motion acting upon it is in different circumſtances. By this Dr. *Cullen* means, for to thoſe who never heard him on the ſubject ſome illuſtration of the text may not be unacceptable,

acceptable, that the different temperature of different animals is owing to a difference of the vital principle, inſomuch, that the velocity of the blood may be the ſame in a frog, as in a man, yet in conſequence of the different vital principle, the heat produced be different. The facts, upon which he ſeems to lay greateſt ſtreſs are, that neither where the ſurrounding medium conſiderably ſurpaſſes the temperature of the living body, nor where it is far below it, is there any ſenſible change in the heat of animals. Theſe, and ſome ſimilar facts, ſeemingly countenance his hypotheſis, but they will, it is preſumed, be hereafter explained on principles more obvious, and conſiſtent with the ſimplicity of nature; for while we admire the ſingular ingenuity, which ſtamps every part of the *Cullenian* doctrine, we muſt be permitted to conſider it, in this particular, as founded on a more ſpecious, than ſolid baſis. What juſt grounds have we to imagine the principle of life different in different animals? And how are we to conceive, that the ſame degree of motion ſhould in one claſs of animals always produce a certain degree of heat, and in another claſs as regularly a different? A propoſition of ſuch a nature ſhould, no doubt,

doubt, require the moſt obvious facts and concluſive arguments to eſtabliſh it; but in the preſent inſtance we do not even perceive any probable reaſon from analogy. Beſides, to ſay that the principle of life can generate heat or cold, independent of chymical or mechanical means, is contrary to experience, and ſeems in itſelf abſurd. Upon the whole, from theſe few of the many objections that tend to overturn Dr. *Cullen*'s theory of animal heat, we do not heſitate to account it a mere hypotheſis, and entirely abandon it.

It will not, it is preſumed, be deemed foreign to the preſent queſtion to mention an opinion, lately publiſhed by Mr. *John Hunter* in the Philoſophical Tranſactions *. That ingenious phyſiologiſt, after reciting ſome experiments reſpecting animal heat, aſſerts, that certain animals entirely deſtitute of nerves are endowed with a power of generating their own heat; and this he brings as an *argumentum crucis* againſt the opinion of thoſe, who account the nervous ſyſtem the ſeat of animal heat.

If this be really a matter of fact, it muſt, no doubt, have all the weight which he

* Vol. LXVI.

aſcribes

aſcribes to it; but how much ſoever it may favour my objections to the *Cullenian* doctrine, I ſhall decline taking any advantage of it, till Mr. *Hunter* has offered ſome more ſolid arguments in ſupport of it than bare aſſertions; and till he has ſpecified at leaſt the tribe of animals, in which he met with a phenomenon ſo repugnant to the general laws of the animal economy. No doctrine in phyſiology has hitherto been more univerſally admitted than that all the more perfect animals are provided with a regular ſyſtem of nerves, on which ſenſe, and motion, and life itſelf, immediately depend; and though this ſyſtem be more or leſs perfect, according to the different gradations of life, and may, perhaps, in the very loweſt claſs of animals, be too minute and indiſtinct to admit of ocular demonſtration; yet even this is not to be accounted an incontrovertible proof of the non-exiſtence of nerves in thoſe animals; for all anatomiſts allow, that there are thouſands of nervous filaments, ſo finely interwoven into the compoſition of the more perfect animals of every ſize, as to elude not only the knife and naked eye, but even the beſt optical inſtruments hitherto invented. Since then we admit the preſence of nerves in one

tribe of animals, though we can only perceive them in their effects, what solid reason have we to deny them in another, in which we have the very same evidence: *viz.* certain indications of sense and motion? Besides, is it consentaneous to the known simplicity of nature to suppose that there is a different principle of life in different animals? As therefore all experiments hitherto have shewn the nervous system to be the organ of sense and motion, or seat of the living principle, we must be permitted to reject Mr. *Hunter*'s opinion, as a conjecture, at present neither supported by facts, nor analogy. On what he founds his own judgment, I do not pretend to conjecture; but till such time as he communicates it to the public, it is surely allowable to believe that neither animal heat can be generated, nor the principle of life itself subsist, where there is not a more or less perfect system of nerves.

SECT.

SECT. V.

Dr. Black's *Doctrine of Animal Heat.*

We now come to the last, and perhaps the most ingenious and best supported theory which has ever been proposed on the subject of animal heat: I mean that of Doctor *Black.* That excellent chymist having observed, that not only breathing animals are of all others the warmest, but also that there subsists so close and striking a connexion, betwixt the state of respiration and the degree of heat in animals, that they appear to be in an exact proportion to one another, was led to believe that animal heat depends on the state of respiration; that it is all generated in the lungs, by the action of the air upon the principle of inflammability, in a manner little dissimilar to what occurs in actual inflammation; and that it is thence diffused by means of the circulation over the rest of the vital system *. This opinion is countenanced by many for-

* Maclurg Differt. Phys. de Calore. *Edin.* 1772.

cible arguments, the chief of which ſhall be ſubmitted to the reader, with animadverſions upon them.

In the firſt place, it is at this time pretty generally known to naturaliſts, that a quantity of mephitic phlogiſticated air is conſtantly exhaling from the lungs of living animals. Since therefore atmoſpherical air, by paſſing through the lungs, acquires the very ſame properties, as by paſſing through burning fuel, or by being expoſed to any other proceſs of phlogiſtication, it is obvious that the change, which the common air undergoes in both caſes, muſt be attributed to one and the ſame cauſe: *viz.* its combination with phlogiſton. Thus far then is Dr. *Black*'s opinion ſeemingly well founded.

It has likewiſe been urged in favour of the ſame hypotheſis, that the celerity with which the principle of inflammability is ſeparated in reſpiration, is very cloſely connected with the degree of heat peculiar to each animal. Thus, man, birds and quadrupeds, vitiate air very faſt; ſerpents, and all the amphibious kind, very ſlowly; and the latter, as was formerly obſerved, are of a temperature inferiour to the former, and breathe leſs frequently: but the moſt cogent arguments, which

which have been brought in support of this opinion, are, that no heat is generated till the function of respiration be established, and that the fœtus *in utero* derives all its heat from the mother.

These arguments may, perhaps, on a superficial view of the question, appear conclusive; but a sound reasoner, who shall coolly and impartially weigh every circumstance, will, I am confident, allow that they only afford a very ambiguous and imperfect evidence of the doctrine, which they are meant to establish: and the subsequent animadversions on Dr. *Black*'s theory at large will, it is hoped, suffice to show that it is not only founded on dubious and controvertible principles, but that it is, in every point of light, clogged with insurmountable difficulties.

I. Many and various are the proofs, which evince the improbability of the lungs being the source or elaboratory of animal heat; for though it be granted that there subsists a very striking connexion betwixt the state of respiration and the degree of heat in animals, and that they are even in proportion to one another, yet it by no means ensues, that the former is positively the cause of the latter; for,

for, were that really the caſe, it is obvious that thoſe animals, which are deſtitute of the organs of reſpiration, would generate no heat. That however is not true in fact, for thoſe fiſhes, which are even deſtitute of gills, appear, from various experiments, to be warmer than the ordinary temperature of the element in which they live; an irrefragable proof that the function of reſpiration is not abſolutely neceſſary to the production of heat in animals.

II. If the heat of living animals be generated ſolely in the lungs, two things neceſſarily follow; the firſt, that it can only be communicated to the other parts of the body through the channel of the arterial ſyſtem. The ſecond, that it muſt decreaſe as it recedes from its ſuppoſed center: and a clear and ſatisfactory evidence of both theſe eſſential points will no doubt be deemed requiſite to render Dr. *Black*'s opinion in any degree probable. So far, however, are we from meeting with thoſe poſitive and convincing proofs, which we had reaſon to expect, that we are not preſented with a ſingle plauſible argument in favour of either of the points. On the contrary, it is more conformable to facts, that the venal blood is, if not

warmer, at leaſt as warm as the arterial. Dr. *Stevenſon*, an ingenious and accurate phyſiologiſt, with a view to aſcertain this matter, laid bare the jugular vein and carotid artery of a calf, and then tied them and cut them off at once, in order to let equal quantities of blood flow, in a given time, into veſſels of an equal capacity, in each of which he had placed a well-adjuſted thermometer *. The reſult of the experiment was, That the thermometer immerſed in the venal blood roſe ſeveral degrees above that placed in the arterial. But though it is probable that the difference is not ſo remarkable as that experiment makes it, yet ſeveral reaſons to be hereafter aſſigned, incline me to think that the venous blood inſtead of being colder, as Dr. *Black* maintains, is in fact ſomewhat warmer than the arterial: and what entirely overturns his opinion is, that no experiment, though many have been made, has ever ſhown that the temperature of the blood is in the left ventricle of the heart higher than in the right, which muſt neceſſarily be the caſe, were all the heat of the animal body generated in the lungs ‡.

* Med. Eſſays Edin. *vol.* VI.

‡ Dr. *Cullen* phyſiol. prelect.

III. Ha-

III. Having thus rendered it improbable that the generation of animal heat should be entirely confined to the lungs, we shall venture a step farther, and endeavour to show that the vital fluid, so far from acquiring all its heat in its passage through the pulmonary system, communicates no inconsiderable portion of what it had received in the course of circulation to the atmospherical air alternately entering into that organ, and issuing from it. Various are the arguments, which tend to evince this opinion. Were the blood heated in the lungs, we should certainly need less of their function in a warm than in a cold atmosphere; but we are taught by experience, that when the air is extremely hot, and we wish to be cooled, we breathe full and quick; and that when it is intensely cold, our respiration is slow and languid; which, were the blood heated in the lungs by the action of the air upon it, surely should not be the case. It is therefore more consonant with reason and experience, that the air which we inspire, by carrying off a quantity of evolved phlogiston from the lungs, rather contributes to diminish than increase the heat of breathing animals. Respiration, for this reason, has been very properly compared, by an

an ingenious phyſiologiſt, Dr. *Duncan*, to the blowing of bellows on a hot body *. In both caſes a conſiderable degree of heat is communicated to the air, but in neither, can the air be ſaid to generate any heat: for if it did, the heat of breathing animals ſhould increaſe in proportion to the quantity of air inhaled, and a piece of inert matter, heated to a certain degree, ſhould become hotter by ventilation.

IV. The fœtus *in utero*, according to Dr. *Black*'s hypotheſis, generates no heat. The arguments, by which he ſupports that poſition, how ingenious ſoever they may be, ſeem not to me ſufficiently cogent to produce conviction; and as the queſtion, from its nature, hardly admits any direct experiment, our reaſoning upon it muſt neceſſarily be analogical. Hence ariſes our embarraſsment; for, as the diſcovering of analogies depends on the quickneſs and fertility of fancy, and the truth of all analogical ratiocination on the acuteneſs and nicety of judgment, two powers of the ſoul ſeldom united in an eminent degree, we cannot wonder that arguments of this kind, which to one man ſeem

* Prelect. on the Inſtit. of Med. *Edin.*

unanſwerable, ſhould to another appear futile.

The only plauſible objection to the generation of heat in the fœtus, is the ſuppoſition that it would, in a ſhort time, accumulate ſo as to become incompatible with life.

This argument, however, is more ſpecious than ſolid; for granting that the circulation, which is carried on between the fœtus and the mother, tranſmits very nearly the temperature of her blood, that, by no means entirely ſuperſedes the neceſſity of heat being generated in it. Various reaſons lead to this opinion. It is an axiom that heat, however excited, decreaſes as it recedes from the ſource from which it ſprang: Now, if we admit for a moment Dr. *Black*'s opinion, and believe the heat of animals to be generated ſolely in the lungs, is it not obvious, that before it reaches the *uterus*, paſſes thro' the very minute tubes, by which that organ is connected to the *placenta*, circulates thro' the umbilical veſſels, and pervades the extreme parts of the fœtus, it muſt be too much diminiſhed to ſupport that æquilibrium, which we ſhowed to obtain in every part of the living ſyſtem. Beſides, as the fœtus *in utero* may

may properly enough be accounted a part of the mother, the ſame objections that are brought againſt the generation of heat in it, would hold equally good againſt any being produced in any other part, or organ of her body, except the lungs. But ſuch an infinitude of accurate thermometrical obſervations have evinced the partial increaſe of heat in local inflammations, that no room is left to doubt, that, in every individual part of the vital frame, heat is generated; and if the fœtus be, from any cauſe whatever, liable to topical inflammation, a thing which no phyſiologiſt has ever pretended to deny, what ſhadow of reaſon is there for doubting that ſuch affections are accompanied with the ſame effects, before, as after birth, and conſequently with a partial increaſe of heat?

But though theſe arguments will to moſt appear concluſive, with reſpect to the fœtus having a power of generating heat, yet they will not perhaps ſatisfy all, why it does not accumulate.

If, however, the following circumſtances be allowed their proper weight, they will, it is hoped, remove all doubts reſpecting that matter. In the ſequel of this eſſay, it will appear that the generation of heat may very

properly be confidered as a neceffary confe-quence of the material mechanifm, and func-tions peculiar to animated body; and there-fore muft of courfe very much depend on the actual ftate and peculiar circumftances of the animal. Now the ftate and circumftan-ces of the fœtus are manifeftly fuch, as ef-fectually obviate the generation of heat in any confiderable degree, and of confequence its accumulation. According to Dr. *Black*'s theory, and to that which fhall be hereafter propofed, the heat of animals entirely depends on the quantity and ftate of the phlogifton prefent in the mafs of blood: and from the furvey, which was taken of all the different genera of animals, it appeared that the de-gree of heat peculiar to each is proportion-able to the number of red globules in the circulating fluids.

Dr. *Haller*, who maintains that the red globular parts of the blood are chiefly of ufe to generate heat, becaufe they always abound in proportion to the natural heat of the ani-mal, fubjoins, that in the fœtus the blood is neither red nor denfe. "Apparet (fays he) "in fœtu deeffe fanguinis ruborem, denfita-"tem folidam *."

* Prim. Lin. Phyf.

And

And indeed it appears, from the very accurate experiments of Dr. *Maclurg*, that not only the blood, but also the bile and urine, the most pungent of the animal secretions in the adult, are, in the fœtus perfectly bland, and almost destitute of every sensible quality; and that, on chymical analysis, they give no marks of their containing phlogiston, unless in a very small proportion. Hence we may reasonably conclude, that nature has studiously provided against the maternal blood passing into the circulatory system of the fœtus, before it has undergone all the various changes consequent on percolation, secretion, and stagnation; and the final cause of this wise provision evidently is, to refine and purify the vital stream of all its gross and recrementitious parts, so that none but such as are sufficiently elaborated, and prepared to afford a ready and immediate supply to the solids of the embrio, shall be filtrated into the placenta, through the invisibly minute exhaling vessels of the uterus. Now, if we admit what shall be hereafter shewn, that from the moment the chyle passes into the circulation, the phlogiston which it contains is rapidly passing from a fixed to an active separable state, and perpetually escaping not

only

only from the ſurface of the lungs, but every point of the whole expanſion of the cuticle, we ſhall have little difficulty to conceive how that principle comes to exiſt in ſo ſmall a proportion in the fœtus, and of courſe, why the degree of heat generated is much leſs before than after birth. But there is no need to reſt the matter on the uncertain baſis of plauſibility, when we have the juſteſt and moſt ſtriking analogy, in direct corroboration of what has been laſt urged. The moſt concluſive proofs were formerly brought, that the chick *in ovo*, as ſoon as the circulation is fully eſtabliſhed, is endowed with the power of generating heat. Is it then conſiſtent with the principles of ſound reaſon and analogy, to deny that the fœtus *in utero* is poſſeſſed of a ſimilar power? And if we truſt to the well-known candour and accuracy of Dr. *Stevenſon*, we ſhall no longer entertain any doubt of the matter; for he poſitively affirms, that a full grown fœtus taken away from the mother, but continuing within the membranes, can live ſeveral hours by its own heat without any aſſiſtance from the lungs *. This fact at once overturns Dr. *Black*'s hypotheſis,

* Edin. Med. Eſſays. *Vol.* VI.

ſince

ſince it affords the cleareſt evidence of heat being generated in the animal machine before the lungs come into play. In fine, tho' breathing animals are the warmeſt, there is not ſo much reaſon for ſaying that they are ſo, becauſe they breathe, as that they breathe becauſe they are warmeſt.

V. It will not, it is preſumed, be unacceptable to the reader, to conclude the review, which has been taken of Dr. *Black*'s theory of animal heat, by a recapitulation of the chief reaſons which induce me to reject it. In the foregoing diſcuſſion, it was only neceſſary to evince one of two things; either that reſpiration takes place without animal heat, or animal heat without reſpiration: With reſpect to the firſt propoſition, as life itſelf, and every vital function immediately depend on a certain degree of heat, it could not in the nature of things admit of proof. On that account, all our reaſoning has been directed to the proof of the ſecond, and it is hoped not without the deſired effect. From adverting to the incubation of eggs, we learned that the chick *in ovo* generates heat for ſome time before it can have any communication with the atmoſpheric air. We

We have alſo ſeen that a full-grown fœtus may ſubſiſt ſeveral hours after birth without reſpiration, and preſerve its natural temperature. Are not theſe convincing proofs that the generation of heat takes place in animals prior to the function of reſpiration? And does it not appear from thoſe caſes related by *De Haen*, and formerly mentioned, in which the thermometer roſe ſeveral degrees after death, that is after the lungs had ceaſed to play, that animal heat is generated without the aid of reſpiration? But even granting theſe arguments to be too ambiguous to produce conviction, a little attention to the phænomena of animal heat will evince the repugnancy of Dr. *Black*'s doctrine to the known laws of the animal machine.

Whilſt man enjoys perfect health, his temperature is ſtable, and every where invariably the ſame; but no ſooner has a local inflammation invaded any part of his body, than, in the ſpot affected, the thermometer indicates a partial increaſe of heat: And as no part of the animal frame is exempt from topical inflammations, a preternatural and partial degree of heat is found ſometimes in one place, ſometimes in another, and very often in the extremities. Since therefore thoſe

those parts of the human fabrick, which are most remote from the lungs, are frequently sensibly hotter than they are, is it not absurd to account that organ the center and sole elaboratory of the heat of animals?

Such are the animadversions which occurred to me on an attentive consideration of Dr. *Black*'s hypothesis; and, I flatter myself, they will suffice to convince the impartial reader that, though his opinion be in some respects just, and in all ingenious, it is by no means adequate to the solution of the phænomena of animal heat.

It would, I think, be paying a sorry compliment to a man of Dr. *Black*'s truly liberal and philosophic turn of mind, to offer any apology for having thus freely canvassed his opinion; which, though never published, is well known to all who have attended his lectures. Besides, I had once the satisfaction of a private conversation with the Doctor on the subject, in which he explained to me his sentiments at such length, that I have every reason to believe, that they are here delivered with sufficient accuracy and perspicuity.

I should not, however, have been so particular in the refuting of Dr. *Black*'s hypo-

thesis, had I not considered a careful discussion of his doctrine as one of the best means of developing some of the radical principles of my present inquiry.

End of PART FIRST.

A PHI-

A PHILOSOPHICAL ENQUIRY INTO THE CAUSE OF ANIMAL HEAT.

PART II.

CHAP. I.

THEORY *of* ANIMAL HEAT.

THE reasons, which induced me to reject the sentiments of former physiologists, being now assigned, it is necessary to give an account of that Theory, which will, it is hoped, solve this important question. Here then my difficulties com-

mence; for who can presume to think himself capable of presenting to the public a system complete in all its parts? Or were any one adequate to the task, would he dream of escaping the misrepresentations of envy, the misapprensions of errour, or the shafts darted by the pride of those, who, having attained the pinnacle of scientific fame, endeavour to depreciate their opinions, who dare to think for themselves. With the utmost diffidence, therefore, I proceed to mention the hypothesis with respect to animal heat, which to me seems incumbered with fewer objections, more conformable to the simplicity of nature, and more consentaneous to sound philosophy, than any, which it has been my fortune to hear. My idea is briefly this: "That the "subtle principle, by chymists termed *phlo-* "*giston*, which enters into the composition "of all natural bodies, is, in consequence of "the action of the vascular system, gradual- "ly evolved throughout every part of the "animal machine, and that, during this "evolution, heat is generated." This opinion was first, I believe, explicitly delivered in the university of Edinburgh, during the winter of 1774-5, by my learned and ingenious friend Dr. *Duncan*, whose lectures on the

the Inſtitutions of Medicine I had, at that time, the pleaſure of hearing; but the ſame notion, or one very nearly ſo, had occurred to ſeveral of the moſt attentive obſervers of nature, as appears from the following, and ſeveral other paſſages in Dr. *Franklin*'s invaluable works. "I have been inclined to "think that the fluid *fire*, as well as the "fluid *air*, is attracted by plants in their "growth, and becomes conſolidated with "the other materials of which they are form-"ed, and makes a great part of their ſub-"ſtance; that when they come to be digeſt-"ed, and ſuffer in the veſſels a kind of fer-"mentation, part of the fire, as well as part "of the air, recovers its fluid *active* ſtate "again, and diffuſes itſelf in the body, di-"geſting and ſeparating it; that the fire, ſo "re-produced by digeſtion and ſeparation "continually leaving the body, its place is "ſupplied by freſh quantities, ariſing from "the continual ſeparation; that whatever "quickens the *motion of the blood* in an ani-"mal quickens the ſeparation, and re-produ-"ces more of the fire, as exerciſe; that all the "fire emitted by wood and other combuſti-"bles when burning, exiſted in them before "in a ſolid ſtate, being only diſcovered when

"ſepa-

"separating; that some fossiles, as sulphur, "sea-coal, *&c.* contain a great deal of solid "fire, and that in short, what escapes and is "dissipated in the burning of bodies, besides "water and earth, is generally the air and "fire that before made parts of the so- "lid *."

An opinion, very similar to the above, had several years before Dr. *Franklin*'s time been proposed to the Royal Society by Dr. *Mortimer* ‡. This learned physician, from considering the excitation of heat by fermentation and mixture, where there is evidently no introduction of adventitious particles of fire, was led to infer that the elements of fire lay hid, or dormant, in bodies; and that the air, which most substances both solid and fluid contain, being set at liberty, by its elasticity, excites into motion the latent particles of fire, and generates heat; and therefore, as the animal fluids not only contain a large proportion of the phosphoric principle, or sulphur, in a quiescent state at least, but likewise a considerable quantity of air, he concluded that the generation of heat, in the vital frame,

* Last Edit. of his Letters, *p.* 346.

‡ Philosoph. Transact. *Vol.* XLV.

was

was the neceſſary conſequence of the particles of phoſphorus and air coming into contact; and this, he ſuppoſed, effected by means of the circulation, alleging even that, if it were not for the ſuper-abundance of aqueous humours in animals, fatal incenſions would frequently happen. This hypotheſis is no doubt very ingenious, and as far as reſpects the exiſtence of phoſphorus, or phlogiſton in the blood, and its extrication, in conſequence of the inteſtine motion induced by the action of the heart and arteries, it is juſt. But as to the air, preſent in the animal juices, being anyways inſtrumental in the production of the heat of animals, there does not appear a ſhadow of proof. It is however obvious, that both Dr. *Franklin* and Dr. *Mortimer* were of opinion, which is all I wiſhed to evince, that the heat of living animals is produced by an evolution of the phlogiſtic fluid, and that this evolution depends upon the action of the circulating powers. Though it has been rendered highly improbable that the ſimple conquaſſation of homogeneous fluids ſhould generate heat, there is no room, I think, to doubt, that the motion of the parts of ſeveral *heterogeneous* fluids upon

upon one another may induce ſuch changes, as ſhall be accompanied with heat.

The churning of milk affords the cleareſt evidence of that poſition; for it is obvious on that occaſion, that when heat is generated, a fermentation takes place, a reſolution goes on, and new productions are formed. The change of temperature is therefore not to be aſcribed to the mechanical motion of the integrals, but to the change of mixture. Now, if ſimple agitation be ſufficient to produce thoſe various effects upon milk, a fluid in every reſpect analogous to the blood, may we not with a high degree of probability ſuppoſe, that the ſtrong and unremitting action of the powers of circulation upon the general maſs of blood is attended with ſomewhat ſimilar effects.

If, according to the common opinion, analogy, next to experiment, be in philoſophical inveſtigations the ſureſt guide, it muſt always influence the judgment in proportion to the ſtrength of the reſemblance, and the number of circumſtances in which ſimilarity is found; and here, where the coincidence in every reſpect is ſo ſtriking, analogical arguments cannot fail to ſilence all our doubts.

Ganbius

Gaubius imagines that the red globules are of an oily nature, and therefore it is consistent with his chymistry to suppose that they are better calculated for the generation of heat.

Haller, on the other hand, convinced that there must be something in the nature of our fluids peculiarly adapted to that purpose, considers the iron present in the blood as a matter very fit to enter into the vibrations, which he supposes necessary to the production of heat. As I could not enter into a minute discussion of the merits of these different hypotheses, without overleaping the boundaries of this essay, I shall rest contented with observing, that both these great oracles of physic agree in one very material point, which is the ascribing of the heat of animals, and the colour of their blood, to one and the same cause; for, while M. *De Haller* supposes both to depend on a small portion of iron discoverable by a chymical analysis of the blood, Professor *Gaubius* as confidently imputes them to a quantity of phlogiston present in that fluid, as appears from the following, and many other passages in his works. " A redundancy of red blood not

"only indicates the preſence of an unuſual "proportion of phlogiſton, but ſuperinduces, "with the concurrence of exciting cauſes, a "preternatural degree of heat, expands the "fluids, and occaſions inflammations *."

Now, the ſcope of the preſent inquiry is to prove that the principle of inflammability is actually preſent in the blood, and that it is the efficient cauſe of heat. But the taſk will be the more arduous, as the laſt-named philoſopher, whoſe genius and accuracy are well known to the learned, has ſimply given the fact, without mentioning by what he was firſt led to the diſcovery of it, or even the grounds on which he aſſerts it.

Amidſt the various opinions offered on the ſubject of animal heat, Dr. *Black*'s approaches the neareſt to that which I wiſh to eſtabliſh; though, at the ſame time, in many very material points, it is widely different: for while he conſiders reſpiration as abſolutely neceſſary to the generation of heat, I look upon it,

* "Rubri ſanguinis exceſſus cum phlogiſton in ſanguine abundans notet, quavis occaſione nociva, caloris augmenta immodicas expanſiones, inflammationes creat." *Pathologia.*

as the chief means employed by nature for diminiſhing the heat of animals; and that poſition I truſt hereafter to evince in the cleareſt manner. The theories however agree thus far, that they both aſcribe heat to the agency of the principle of inflammability; but whilſt that ingenious philoſopher confines the generation of heat to the lungs, and attributes it to the action of the air upon the phlogiſton, I maintain that it does not depend on any permanent ſtate in the component parts of the animal fluids, but is a neceſſary conſequence of the conſtant progreſſive mutations of the maſs of blood; or of that peculiar vital proceſs uniformly carried on in every part of the ſanguiferous ſyſtem, by which the phlogiſtic fluid is by degrees evolved, and in the act of evolution exhibits the phænomena of animal heat.[l] This opinion then very evidently differs in two material circumſtances from that propoſed by Dr. *Black*.

For Firſt, he confines the production of animal heat to a particular ſeat, or fountain; but this ſuppoſes it to be generated in every part of the machine, where the circulation is carried on. And ſecondly, according to him, the heat excited is owing to the action of the

air on the phlogiſton already evolved and extricated from the blood; whereas, if the theory I propoſe be well founded, it is the immediate effect of the evolution of that fluid, or its actual tranſition from a fixed to a looſe and ſeparable ſtate. It will, it is hoped, be found that this opinion is by no means obnoxious to any of the objections which have juſtly been made to that of Dr. *Black*; and, as the generating cauſe here extends equally to every part of the animal, it affords a ready ſolution of that very ſtriking phenomenon, the equality of heat all over the body; a fact altogether inexplicable on his hypotheſis. It likewiſe obviates what has been ſo often objected to that theory, that heat takes place prior to reſpiration, as in the chick *in ovo*, and is frequently partial, as in topical inflammations. In fine, it is hoped, that the proof of the ſubſequent propoſitions will be a full demonſtration of the truth of the theory, which I wiſh to eſtabliſh.

1ſt. The blood contains phlogiſton.

2d. The action of the blood-veſſels evolve phlogiſton.

3d. The evolution of phlogiſton is attended with heat.

4th. The

4th. The heat, thus generated, is sufficient to account for the heat of living animals.

5th. The most striking phænomena of animal heat evince the truth of those propositions.

CHAP.

CHAP. II.

The BLOOD *contains* PHLOGISTON.

BEFORE we proceed to the proof of this propoſition, it will not, I preſume, be accounted foreign to the ſubject, to premiſe a conciſe account of the more general properties of phlogiſton.

The ancients had no diſtinct idea of phlogiſton. They confounded it with the compounds, into which it only enters as a conſtituent: They even miſtook its unity, and believed it in different ſubſtances to differ in its nature. But it is now almoſt univerſally taught and believed by chymiſts, that there is an elementary principle of inflammability, or one particular ingredient of the ſame nature in all inflammable ſubſtances; and that the diverſity found in bodies, with reſpect to their capability of being inflamed, ariſes merely from the manner in which that principle is combined with the other component parts. * The diſcovery of this important

* Mem. de l'Acad. des Sciences 1713.

truth

truth was juſtly reſerved to reward the labours of modern philoſophers, among whom the illuſtrious *Stahl* hath contributed moſt to the advancement of the hermetick art. This philoſopher, whoſe genius equalled his induſtry, ſeems to have taken in at one view the almoſt boundleſs range of chymical phænomena, and by maturely weighing and judiciouſly collating them, he invented the beſt and moſt ſatisfactory theory that has ever been publiſhed; a theory, whoſe leading doctrines, inſtead of being invalidated by time, the ſole impartial teſt of ſyſtems, are more and more confirmed by the various diſcoveries, which are daily made in this uſeful branch of phyſicks. But to return to my ſubject: The phænomena of inflammation depend upon the ſeparation of the phlogiſtic fluid; for it being once diſſipated, what remains of the body proves no longer a ſource of heat, but is on a footing with all fixed incombuſtible ſubſtances, which are diſtinguiſhed from inflammables, by their not ſuffering any permanent change from heat, and by readily tranſmitting it to other bodies, in the exact quantity in which they themſelves received it. In many caſes, phlogiſton may

be

be restored to the substances deprived of it, by their being mixed properly with inflammable matters, which, for that purpose, may be taken either from the animal, vegetable, or fossil kingdoms.

But when we attempt to form an idea of the common principle of inflammability, and inquire into its nature in a separate state, we find it a question involved in the deepest obscurity: for it can only be separated from one body by being forced to unite with another, to which, in certain circumstances, it has a greater affinity. In actual inflammation, there is reason to think that it leaves the body in a pure and active state; and as, during its separation, nothing is perceived to issue from the inflamed substance but a stream of fire and light, if any judgment be formed from what we see, we must conclude that phlogiston is fire and light, or a certain subtile elastick fluid, upon the modifications of which the phænomena of heat and light immediately depend *. There are many striking facts, which tend to evince the truth of this proposition. It is well known to chymists, that several bodies

* Dr. *Black*'s Lectures on Chymistry.

after

after being deprived of the phlogistick fluid, do merely, on being exposed to the solar rays, attract a quantity of that principle, and fix it in such a manner, as to produce upon them the same effects as would have attended their union with an inflammable substance. It is therefore, I apprehend, allowable to infer, that the light of the sun communicates phlogiston to bodies; and that the same matter, which in a separate state constitutes fire and light, when modified in bodies is the cause of their inflammability; I mean, that actual inflammation is the appearance that the decomposition of the phlogistic fluid, which I account the material cause of fire and light, produces.

It is somewhat remarkable that Dr. *Priestley*, after offering several arguments to evince the identity of phlogiston with the electrick matter, and even allowing light and that principle to be the same thing, should still be so strongly attached to the improbable notion of an intense vibratory motion of the inert particles of bodies, being alone sufficient to excite heat, as to affirm that heat has no more connexion with the phlogistic fluid, than it has with water, or any other constituent part of bodies. This ingenious philo-

ſopher has not duly adverted to what he himſelf advances in the ſame paragraph, or he certainly would have drawn a very different concluſion; but leſt I ſhould miſapprehend his meaning, I ſhall ſubmit his own words to the judgment of the reader.

"The difference between the ſubſtances "which are called inflammable, and others "which alſo contain phlogiſton, may be this; "that in the former the *heat*, or the vibra-"tion occaſioned by the emiſſion of their "own phlogiſton, may be ſufficient to occa-"ſion the emiſſion of more, till the whole "be exhauſted; that is, till the body be re-"duced to aſhes. Whereas in bodies, which "are not inflammable, the *heat* occaſioned "by the emiſſion of their own phlogiſton "may not be ſufficient for this purpoſe, but "an additional heat *ab extra* may be neceſ-"ſary *." Theſe are Dr. *Prieſtley*'s own words, and I appeal to every thinking reader, if they do not imply the neceſſity of the phlogiſtic fluid to the production of heat.

Since then it is apparent that heat is ſo intimately connected with the principle of inflammability, that it is impoſſible to avoid

* Exper. on Air, Vol. I. *p.* 281.

diſcerning

discerning the relation which the one bears to the other, they may very justly be considered as cause and effect. But nothing farther shall now be urged, as I shall have occasion to enlarge a little more on the subject, when I treat more particularly of the cause of heat.

It is a singular but a well-known fact, that almost all bodies on being deprived of their phlogiston, instead of suffering any diminution of their weight, have it, on the contrary, considerably increased. This has been to many an unsurmountable objection against the existence of an inflammable principle; and indeed to all, who are not conversant in chymical experiments, it may appear absurd to say that a body becomes heavier on being deprived of one of its constituent parts: yet, however paradoxical this may at first sight appear, there are so many facts that tend to establish it, and which admit of no other explanation, that we find ourselves under the necessity of believing it. All metals on being calcined, that is, deprived of their phlogiston, have their weight considerably increased. A pig of lead weighing a hundred pounds will, when calcined, weigh a hundred and ten pounds, *&c. &c.*

This excess, or augmentation of weight, in metallic calcinations, has been long a noted problem in chymistry. As early as the year 1630, some essays were published by M. *Rey*, expressly on this subject. This author attributes the increase of weight to the adhesion of the denser part of the air to the calx, while the more subtle part of that fluid, which prevents its adhesion to other substances, is separated during the calcination *. Since the time of M. *Rey*, various hypotheses have been offered on the subject; some remarkable for their singularity, others for their subtlety; and all for their insufficiency to afford a complete and satisfactory solution of the question. *Boyle* ‡, *Musschenbroeck*, *Lemery*, *Buffon*, and other philosophers, have endeavoured to show that fire is of a ponderous, gravitating, nature, and attributed the acquisition of weight in calcination to the absorption of igneous particles. M. *Lavoisier* hath lately revived Dr. *Rey*'s opinion, and adduced several ingenious experiments to prove

* Essays sur la recherche de la cause pour laquelle l'etain et le plomb augmentent de poids quand on les calcine.

‡ De ponderabilitate flammæ.

prove that this phenomenon depends solely on the quantity of air that is absorbed by metals in the act of calcination. But this explanation of the matter, how plausible soever it may seem, appears to me obnoxious to many weighty objections. The chief arguments in support of this opinion are, that air is indispensably necessary to the calcination of metals; that it is diminished by the operation; and that this diminution cannot be carried beyond a certain proportion to the whole quantity of air employed. To each of these I shall briefly speak.

1°. The absolute necessity of air to calcination has been evinced beyond the possibility of doubt; but the reason of it is still a subject of dispute among chymists. There are few, if any, substances in nature, which either alone or in mixture may not be decompounded by the action of fire; but the decomposition of some is accompanied by flame, of others, by ignition only: In both, however, it depends on the separation and dissipation of phlogiston; and the diversity observed in the phænomena proceeds entirely from the greater or less quantity of that principle and the more or less fettered state, in which it is present. Calcination is therefore justly

justly considered by chymists as a species of combustion, or slow inflammation, and air, on the same account, is necessary to both operations. But why is it necessary to either? The reason assigned by *Boerhaave* is, that air, by its weight and elasticity, keeps the flame constantly applied to the combustible body, and thus increases their contact *. M. *Macquer* imagines, that air materially concurs to the production of flame, and makes a part of it ||. Both these great masters of the hermetic art, seem to have mistaken the true principles on which the phenomenon ought to be explained. Experiments without number leave no room to doubt that it is neither by the weight, nor elasticity of air, nor by being a necessary pabulum or food of flame, that it contributes to inflammation, but solely by the mutual attraction subsisting betwixt it and the phlogistic fluid. The truth of this position is sufficiently apparent from the changes produced on air, which has served to the combustion or calcination of bodies.

2*do*. The most obvious and uniform alteration, which air suffers in the calcination of metals,

* Elem. Chym.

|| Diction. de Chimie.

metals, is a diminution of its bulk. This by Dr. *Prieſtley* and M. *Lavoiſier* has been attributed to an abſorption or precipitation of part of that elaſtick fluid: and the chief argument on which they ground their opinion, is the proportional diminution of the ſpecifick gravity of the air. A thouſand experiments indeed have taught us that atmoſpherical air, on being either by inflammation, calcination, or any other proceſs, impregnated with phlogiſton, ſuffers a real contraction of its dimenſions; and inſtead of becoming on that account heavier, it becomes conſiderably lighter, and this, in proportion to the quantity of phlogiſton with which it is charged. But various arguments tend to ſhow that this diminution of ſpecific gravity doth not ariſe, as the above-mentioned philoſophers pretend, from the precipitation of fixed air. It appears, from ſeveral of Dr. *Prieſtley*'s own experiments, that both the diminution of bulk and weight happen to air originally produced from inflammable air, and even from nitrous gas, which contains no fixed air *; an irrefragable proof that the phenomenon doth not depend on the cauſe, to which that experi-

* Exper. and Obſerv. on Air.

mentalift, and M. *Lavoifier*, have afcribed it. This conclufion is further corroborated by a feries of experiments inftituted by Profeffor *Black*, with an intent to throw light on the fame curious queftion. It is notorious that metals are calcinable by acids, as well as fire, and fince acids and air accord in nothing but their attraction for phlogifton, it is plain that air contributes in no other fhape to calcination, than by its affinity to that principle; and hence the atmofpherical air may be as completely faturated with phlogifton, as an acid with an alkali: and this is the fole reafon, why air that has fuffered its utmoft diminution, is no longer capable of promoting any further calcination.

Dr. *Black*, to obviate the ambiguity which frequently attends chymical operations by fire, employed the mineral acids in his experimental inquiry into the caufe of the increafed weight of metallic bodies by calcination; and to preclude every chance of fallacy, he got Dr. *Irwin*, whofe name is well known in the literary world, to repeat the fame, and many fimilar experiments. Two of thefe, recited by Dr. *Black* in his academical lectures, will fuffice to fet the matter in the cleareft point of view. "Having (fays he) "weighed

"weighed out two ounces of *aqua-fortis* in-
"to two phials of a conical figure, I set them
"by, in a cool room, for 24 hours, and
"found that they neither lost nor gained any
"weight during that time. I then weighed
"out two *oz.* of quicksilver, one of which was
"gradually added to each of the glasses, and
"after the whole was dissolved, I found each
"solution heavier than before by seven grains,
"and yet all the time of the process, a sub-
"tile vapour was copiously emitted, which
"tinged the upper part of the vessel with a
"red colour, and filled the room with a strong
"smell of *aqua-fortis.*"

Dr. *Black* made a similar experiment with silver, which requires double its weight of *aqua-fortis*, and found that an ounce gained half a drachm, *i. e.* the solution one sixteenth of its weight. These experiments are simple, unequivocal, and, in my opinion, decisive against M. *Lavoisier*'s solution of the question. I have lately repeated them with nearly the same result; but trusting more to Dr. *Black*'s accuracy than my own, have given them in his name.

The conclusions that ought to be drawn from the preceding facts are too obvious to

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require much comment; I ſhall therefore only obſerve that the weight, acquired by the ſolution, could not poſſibly ariſe from a precipitation of fixed air; and that as, inſtead of abſorbing any foreign matter, they were all the while exhaling a very copious vapour, they muſt have ſuffered a very conſiderable loſs in their quantity, whilſt they gained in their weight. We are too the more inclined to believe that the exceſs of weight is not occaſioned by the addition of fixed air to the metallic calx, as the acid of ſulphur, which may be collected after the combuſtion of phoſphorus *in vacuo*, is conſiderably heavier than the phoſphorus was *; and none, I preſume, will contend, that there can be much fixed air in the exhauſted receiver. Abbé *Fontana* has even engaged to prove that there is properly no fixed air in the atmoſphere; and Dr. *Prieſtley* owns that he ſuſpects that to be really the caſe. Be that however as it will, there is certainly no grounds for explaining the fact on M. *Lavoiſier*'s principles. Profeſſor *Brugman* found that a quantity of iron-filings, which weighed one ounce, or 480 grains, before it was expoſed

* Margraaf.

to

to the fire, after eleven calcinations, was increaſed in weight 149 grains *.

It appears, from ſome experiments made by Dr. *Roebuck*, in the preſence of ſeveral of the moſt noted philoſophers in this kingdom, that an iron cylinder weighing 55 pounds, after being heated to a white heat, and allowed to cool, increaſed in weight ſix penny-weights ſeventeen grains ‡. Mr. *Whitehurſt* has likewiſe publiſhed ſeveral experiments on ignited bodies, the reſult of which is in every reſpect ſimilar to the former ‖. The bodies, when heated, became apparently lighter, and after being ſuffered to cool, had their weight very conſiderably augmented. But, as in none of the above inſtances ſo much weight could have been acquired by the abſorption of air, without it being condenſed in Profeſſor *Brugman*'s experiment at leaſt, 700 times more than uſual (a thing highly improbable in ſo intenſe a degree of heat); and farther, as this ingenious chymiſt found that the only condition neceſſary, in order to the augmentation of the weight of iron by fire, is, that its phlogiſton be diſſipated, we have no other

* Magnetiſmus, ſeu de affinit. magnet. obſervat.

‡ Phil. Tranſ. *vol.* LXVI.

‖ *Ibidem.*

way of accounting for this singular phenomenon, than by supposing that phlogiston diminishes the absolute gravity of the bodies with which it is combined. This is no doubt a bold opinion, but will not be esteemed a groundless hypothesis by those, who reflect that merely by restoring phlogiston to such substances as had become specifically heavier on being deprived of it, we reduce their weight to its primary state. This circumstance at least appeared sufficiently cogent to those truly ingenious philosophers, M. *Venel*, M. *Morveau*, and Dr. *Black*, to make them conclude that the increase of weight is owing solely to the loss of that principle *.

* Nothing is more obvious than that the diminution of weight, in consequence of the addition of phlogiston to bodies, is very different from an apparently similar effect produced by the addition of a light elastick body to one of greater specifick gravity. Thus a piece of lead, which by itself would immediately sink in water, if joined to a sufficient quantity of cork will swim on the surface. Here indeed the weight of the lead is seemingly diminished, but is not so in fact; for, on weighing the vessel with the water, lead, and cork, we find that it is exactly the weight of the cork heavier than before. The case is otherwise where phlogiston is added, for 110 pounds of calcined lead are, by the restitution of this principle, reduced to 90 pounds; and this, whether weighed in water or in air.

It

It is not without reason that we are backward in believing what appears to us contrary to the general phænomena of nature; but it is the duty of every lover of truth not to suffer himself to be so much actuated by that strong propensity, which induces us to refer all knowledge to certain principles, as to suppose the laws of nature fewer and simpler than they really are. From this impatient desire of generalizing, has sprung the too hasty reduction of science into systems, which is one of the chief causes that have retarded the advancement of natural knowledge. For, though on a superficial survey of the works of nature, she may seem to have uniformly operated on the same plan, yet, when we contemplate her with more attention, and investigate with more accuracy the mode and system of her operations, we are then no less astonished at the variety of the design, than at the multiplicity of the means of execution.

The ancients expressed less astonishment than we do, at the facts which they could not explain. What to us appears an inexplicable paradox, because it does not perhaps tally with the supposed laws of motion and gravitation, was to them no more than a new phenomenon. They were convinced that

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man can only perceive a few of the most obvious springs employed by nature in carrying on her various operations, and therefore pronounced it presumptuous in him to think of limiting her to a definite number of principles of action. But these sages carried their circumspection and diffidence to an improper length, for by thus discouraging an active spirit of inquiry, they effectually checked the prosecution of natural science.

It is of the utmost importance in philosophy to ascertain, as accurately as possible, the more general powers in nature, and to determine their causes, and trace their consequences; for as the phænomena of nature are infinite, and the faculties of the human mind, particularly the memory, are limited, when these phænomena are considered as unconnected with other facts, they convey but little instruction. The infinite is not the object of science; and therefore, till the laws of nature be known, by a careful observation of individuals, and an accurate induction from them, no progress can be made in natural philosophy. Hence the necessity of collating and connecting corresponding facts, and the advantage of reducing them to certain general principles, and applying these to account for other phænomena;

mena; for thus, by a slow and cautious induction, we may advance to a knowledge of the most general laws that regulate the system of nature. But though we be warranted to consider all the phænomena that we find connected with these general laws, and manifestly depending upon them, as so many facts explained, so many truths known and understood, yet we ought not to overlook such phænomena, as are not reducible to those general principles, but should view them as simple and separate facts, and treasure them up till a more enlarged experience, and more accurate observation lead us to the discovery of the powers of nature, to which they should be referred.

Now, as there are so many concurring observations, and such a multitude of facts, tending to prove that bodies become specifically heavier by being deprived of their phlogiston, does it not necessarily follow, that this principle is exempted from the common laws of gravitation; and that, when modified in bodies, it diminishes their centripetal force. This hypothesis, which is seconded by numberless facts and experiments, affords at once an easy solution of all our difficulties; and, for my own part, I am persuaded

persuaded that it is merely in consequence of association, that we suppose all bodies of a ponderous gravitating nature; for *a priori* it is certainly as easy to conceive that a body should have a tendency from, as to the center; and why not suppose the phlogistick fluid possessed of that particular property, since in a thousand other no less material circumstances, it is essentially different from every constituent of terrestrial bodies.

This supposition is not only rendered highly probable, from phlogiston being the manifest cause of the volatility of bodies, but also from a contemplation of the phænomena of light. That subtile, active fluid, which is nothing else than phlogiston under that form, which renders objects visible to us, is emitted from the sun, with such an amazing velocity, as not to be accounted for on any principles of motion hitherto ascertained by philosophers; but if we allow the phlogistic fluid, under the form of light, to possess the same properties as when modified in bodies, I mean, if we admit that it is endowed with a centrifugal force, or power, contrary to that of gravitation, the problem is at once resolved. It is an indisputable truth, that the terraqueous globe is not the center of gravity

to

to any body in the solar ſyſtem, except the moon; each of the primary planets deſcribe an elliptick curve around the ſun, and are accelerated in their motion as they approach to him, and retarded as they recede from him. Now, if we admit the materiality of light, it ſhould, according to the more general laws of matter, gravitate towards that common center, and inſtead of moving through the vaſt regions of ſpace betwixt the earth and orbit of the ſun, at the rate (according to the *Newtonian* calculation) of fifteen hundred thouſand miles in a minute, its velocity, in conſequence of the centripetal force inceſſantly acting upon it in a contrary direction, ſhould certainly be infinitely leſs conſiderable. But if we ſuppoſe light to have a tendency to recede from the center, the difficulty immediately vaniſhes, and on that principle, we in ſome meaſure can account for its almoſt incredible velocity. It is at leaſt an undoubted fact, that the intenſe quantity of motion, with which light is emitted from luminous ſubſtances, does not depend on any projectile power in them, but on ſomething intrinſic and inherent in the nature of that fluid; for it darts with equal velocity from a glow-worm, and from the great luminary,

by the emanation of whose beams, the whole system of beings around us is illumined and rendered visible.

Before I dismiss the subject it may not be improper to observe, that the opinion, which I wish to establish, is by no means so dissonant from the doctrine of *Newton* as may at first appear. That incomparable philosopher was, from various phænomena, led to conclude that matter is endowed with two opposite qualities, an attractive and repulsive power. Both these he ascribes to the action of the ethereal medium, which, by the power of its elasticity, he supposes to fill up the whole regions of space. It is therefore plain, that Sir *Isaac* not only believed that the particles of matter, when placed beyond the sphere of each other's attraction, possess a centrifugal tendency, but even imagined that property to depend on the ethereal fluid. Whether this tendency of the ether, which I account the same agent as phlogiston, be supposed to proceed merely from its extreme elasticity, or from its being endowed with an inherent power, contrary to that of gravitation, by which it is in an incessant *nisus* or effort to recede from the center, is a circumstance of little importance to the doctrine

proposed:

proposed: but, whatever be the material being or cause of gravitation, it surely were absurd to suppose it of a ponderous gravitating nature; for, on that supposition, a third agent would be requisite to be the cause of its gravity. The substance then on which gravitation depends must necessarily be exempt from gravity. But how is that wonderful operation performed? Is gravity the effect of external impulse, or is it an inherent and essential property of matter? The latter opinion seems at present to prevail, but the former is that to which the great *Newton* inclined *; and indeed to me it appears incumbered with fewer difficulties. Did the accelerated motion of falling bodies entirely depend on the mutual attraction betwixt them and the center of the earth, the accelerations in their velocity should be proportioned, not to the time elapsed in descending, but to their approximation to the attracting power. But as the contrary happens universally; and farther, as bodies, *in vacuo*, of the most unequal gravity descend with the same velocity, there is a strong presumption that the chief

* Optics, *quer.* 21.

cause of gravity is an external impelling power; and is, according to the *Newtonian* system, effected by the subtile medium pressing on the surface of gross bodies, and impelling them into those spaces where its density is least. That all bodies, however, according to the quantity of matter they contain, and the nature of their constituents, exert to a certain distance an attractive force; I readily allow, and even believe, that it contributes not a little to the phenomenon of gravitation as well as cohesion: but this will more clearly appear, when I treat of the cementing principle in bodies.

Several other particulars, favourable to the opinion I have hazarded, on the diminution of weight by the addition of phlogiston, might here be mentioned, which are reserved to a subsequent part of this essay, when I hope to evince the identity of the *Newtonian* ether, the electrical *aura*, *materia subtilis*, fire and light, with that elementary principle, an agent which, from the most obvious phænomena, appears to pervade all nature, to fill all things, and to communicate to them whatever life and activity they possess. By the ancient and more ignorant chymists, it was denominated the *sulphur principle*, and has by

by the modern been ſomewhat improperly ſtyled *phlogiſton*, which ſignifies *inflammable*; I ſay improperly, for it is not inflammable in itſelf, any more than fixed air is effervеſcent; but it is a conſtituent part of a compound, which being ſet looſe, cauſes by its flight the phænomena of inflammation, juſt as by the eſcape of fixed air, what is called *effervеſcence*, is produced. *Stahl* and Dr. *Black* have, with more propriety, called it the *principle of inflammability*; but, in order to avoid all appearance of ſtiffneſs and affectation, I have made, and ſhall make uſe of both names indiſcriminately.

That ſuch a principle, as has been juſt now deſcribed, exiſts in the animal body, is demonſtrable from numberleſs facts and experiments. We are convinced of the preſence of the phlogiſtic fluid in all animal ſubſtances, not only by their readineſs to be inflamed, but alſo by the ſureſt of all teſts, their reſtoring that principle to ſuch bodies as have been deprived of it. A variety of other circumſtances tend to corroborate the ſame fact. It is well known that animal ſubſtances, particularly during the putrefactive proceſs, frequently appear luminous in the dark; and that, I maintain, is ſolely to be attributed to the

the extrication of the principle of inflammability under one of its least ambiguous forms, that of light. This conclusion, it seems allowable to deduce from the large quantity of inflammable air, which may be collected, as it rises, from putrifying masses of animal matter. Many experiments afford the clearest evidence of its presence in the common mass of blood. From blood recently drawn an *halitus* or vapour is observed to escape, which being received into proper vessels and condensed, is found to be a limpid water impregnated with native phlogiston. This vapour is more pungent in carnivorous than granivorous animals, and even instances are not wanting in the annals of physic, in which it proved fatal. On chymical analysis, M. *De Haller* found it to be the same as the matter of perspiration; and that indeed is what, *a priori*, we should have supposed *; for as neither it, nor perspiration, are of an acid or alkaline nature, their pungent and other sensible qualities evidently depend on a highly attenuated oily matter, of which phlogiston is the chief ingredient. There is at least no doubt, but this is the case with re-

* Element. Physiolog.

spect

ſpect to perſpiration, as appears from the ſtrong ſcent that is conſtantly ſmelt after profuſe ſweats in fevers; and alſo from dogs being at all times able, by means of this odour, not only to diſtinguiſh one ſpecies of animal from another, but an individual, from every other of the ſame ſpecies; and all chymiſts agree that ſmell depends upon the phlogiſtick fluid, varying only by means of the different ſubſtances with which it happens to be combined. But what puts this opinion beyond all diſpute is, that if a piece of ſilver be kept in contact with our ſkin, it will be as ſoon blackened, as if it had been expoſed to the principle of inflammability eſcaping from bodies in any phlogiſtic proceſs; and what is equally concluſive, Dr. *Prieſtley* found, on putting pieces of the craſſamentum of ſheep's blood into dephlogiſticated air, that in the ſpace of twenty-four hours, ſo much phlogiſton was communicated to this pureſt of all kinds of air, that it became unfit for reſpiration *.

It is therefore not only evident that the vapour, which eſcapes from blood recently drawn, is the ſame with that, which is con-

* Philoſoph. Tranſ. *vol.* LXVI.

ſtantly

ſtantly flying off from every pore of the vital frame, but alſo that the principle of inflammability enters as a chief ingredient into its compoſition, and conſtitutes all its ſenſible qualities. Since then ſo many ſtriking facts concur in evincing the preſence of phlogiſton in the animal body, and as nothing nouriſhes but what contains it, it will not, I apprehend, admit of a doubt that by the digeſtive powers it is extricated from the aliment, under the form of an emulſion, is taken up by the lacteals, carried into the courſe of circulation, and conſequently that it abounds in a particular manner in the vital ſtream.

CHAP.

CHAP. III.

The Action of the Blood-Vessels *evolve* Phlogiston.

IT is not enough for my purpoſe to have evinced the preſence of the phlogiſtick fluid in the circulating maſs, in ſo fixed and fettered a condition as merely to conſtitute a component part of it, but it is alſo neceſſary to ſhew that the ſame principle is evolved by the action of the blood-veſſels, and exiſts in the animal body in a ſeparable and active ſtate. It will not, I foreſee, be an eaſy matter to bring a full demonſtration of this propoſition from any ſingle fact; but from an attentive conſideration of a multiplicity of circumſtances taken together, I hope to render it highly probable, not to ſay abſolutely certain.

If it can be eſtabliſhed as a matter of fact, that the colour of the animal fluids is intimately connected with the ſtate of their phlogiſton; and eſpecially, if it be ſhown that

colour absolutely depends upon that principle, there will be very little room to doubt that the phlogiston present in the blood is evolved, and brought into a more active state by the action of the circulating powers. Since all the fluids of the body are primarily derived from the common *mass* of blood, very properly so called, as either formally or materially containing every part of them; and since they by consequence, how differently soever they may be modified by the various processes of secretion and stagnation, which the purposes of the economy require them to undergo, partake universally of its nature, it will certainly sufficiently establish my position, if any one fluid in the body can be produced, the colour of which is proved to depend on the proportion and condition of of the principle of inflammability, which it contains; and Dr. *Maclurg*, by a course of very ingenious and accurate experiments, has in the most satisfactory manner demonstrated the bile to be such a fluid.

The bile is a very remarkable secretion in the animal economy, and its nature and use have been a subject of great dispute. It is a fluid somewhat viscid, of a deep yellow colour, inclining to green; is exceedingly bitter,

ter, has a ſmell peculiar to itſelf, and a ſlight degree of aromatic flavour; it is neither acid nor alkaline when freſh; it readily unites with water, and thus combined mixes with oil, and is in every reſpect a powerful penetrating ſoap. The bile, at firſt, appears homogeneous, and undergoes no ſpontaneous ſeparation before ſome degree of putrefaction has taken place. When dried, it flames in the fire, and burns almoſt all away; it diſſolves all gums and reſins, and painters, for this reaſon, uſe it in their pigments. The viſcidity and other ſenſible properties of the bile vary much according to age. In the fœtus it is deſtitute of taſte and ſmell; in the adult it has both in a high degree. I have premiſed this ſhort account of the bile for the ſake of thoſe, who may not be much converſant in phyſiology.

Dr. *Maclurg* has written a very ingenious treatiſe upon the bile, and ſeems to have grounded all his deductions on facts and experiments. He found that ſtrong ſpirit of vitriol produced an inſtantaneous coagulation; but the coagulum ſoon diſappeared, being diſſolved by the acid, and the mixture aſſumed a green colour. Strong ſpirit of nitre produced at firſt a coagulation, and then a

whey-coloured ſolution, which depoſited a whitiſh ſediment. By weak nitrous acid applied to the bile, an elaſtick vapour of a blue flame colour was produced, which turned to green, provided that the phial was kept ſhut *. From theſe experiments we may infer that the bile contains a large proportion of coagulable matter; and, as that part is highly inflammable and not ſoluble in water, but is partly diſſolved by ſpirit of wine, it has been generally accounted by phyſiologiſts of a reſinous nature. It is however much more probable, that the coagulable part of the bile is pretty much the ſame with the coagulable lymph, or gluten of the blood, for it is readily coagulated by acids and alcohol, and may be ſeparated from the other ingredients of this ſecretion.

The foregoing experiments ſhow, that the colour of the bile may be perfectly extracted by the dulcified ſpirit of nitre; for the coagulum, which falls to the bottom, is on that occaſion white; and the green colour, which the mixture aſſumes, is evidently produced by the ſolution of phlogiſton in a looſe or ſeparable ſtate, with that acid. This opinion

* Eſſay on the Bile.

is

is strongly countenanced by what is observed to occur in the solution of several metals by means of the pale spirit of nitre, which uniformly receives a blue colour inclining to green, from the loose phlogiston uniting itself to the acid, and changing it to the volatile nitrous acid. On this occasion it is likewise observed, that if the flame-coloured elastic vapour be confined and obliged to mix itself with the solution, the green colour returns, as in the case of the bile, which does not happen, when it is allowed to escape.

Though it cannot perhaps be shown that the colour of bodies in every case depends on the principle of inflammability, yet it may in general be remarked that all substances, on being deprived of it, either lose their original colour entirely, or have it materially altered. This is the reason why the degree of whiteness in the calces of metals is the surest indication of the degree of calcination. The same notion is farther illustrated by a remarkable circumstance, which all chymists must have observed in comparing the two spirits of nitre. The one is of a red colour, emits a suffocating steam, and tinges the phial of an orange colour; the other is pellucid, and emits

emits no vapour; but on adding a few drops of ether, steams immediately begin to arise, and the colour deepens into red: an effect, which very evidently proceeds from the phlogiston contained in the ether. Dr. *Priestley* recites several experiments, which shew that it is only when the nitrous acid becomes, in some measure, phlogisticated, that it assumes a deep orange colour; and that the less phlogiston it has, the paler it always is *. I shall afterwards have occasion to prove that vegetables owe their colour to that principle; and is it not highly in favour of the opinion in general, that the electrical aura, which I account the phlogistic fluid in its purest state, shews as beautiful prismatick colours, as any that are exhibited by the rays of the sun?

Several physiologists have ascribed the colour of the bile to a small portion of iron discoverable in it by chymical analysis; but there is no good reason for admitting that supposition; for a solution of galls, added to a mixture of bile and spirit of nitre, strikes no black colour, which it certainly would do, were its colour owing to the iron present in it. The colouring matter is evidently the same

* Observ. on Air, vol. III.

same with that, which gives the pungent bitter taste, since we find both colour and taste extracted from it, and restored to it, by the same menstruum; and both, we presume, are ultimately to be ascribed to the wonderful and universal principle, to which metals owe their splendour, ductility and elasticity, and to which the most active part of vegetables (their essential oils) owe their peculiar distinguishing properties; to the phlogiston in short, without which, what we call body would remain an inactive, passive, incoherent calx, but which is of so subtile a nature, that it is only discovered by the effects produced on the substances with which it is combined. It is from those effects, we learn that it is present in the bile; and from its phænomena with the mineral acids, that it is in a state similar to that, in which it is found in metals.

Hence we may consider the bile as a compound fluid; its fluidity depending on water, its colour and taste upon a large proportion of the principle of inflammability, and its viscidity on the coagulable lymph. The blood and the bile therefore are in their nature analogous; but the union of parts seems more complete in the bile, as no spontaneous sepa-

ſeparation takes place. There is likewiſe a difference of colour; but numberleſs facts and experiments tend to ſhow that the principle of colouring matter in the blood is the ſame as in the bile. The phlogiſtic fluid indeed, on which both depend, appears to be in a ſtate ſomewhat more fixed and concentrated in the bile, than in the blood, from the red globular part of which it is prepared; but if we reflect that ſtagnation and fermentation very frequently produce new bodies, we certainly cannot be ſurpriſed to find that they effect very ſtriking changes.

The colour of the blood has been generally aſcribed to the quantity of oil which it contains *; and indeed, from my manner of explaining the matter, the colour and oily part are no doubt intimately connected; for tho' I do not find that the red particles, on a chymical trial, afford more oil than the coagulating lymph, yet colour, which is enough for my hypotheſis, is well known to depend more on the particular ſtate of phlogiſton, than on the quantity preſent. This appears from an experiment related by Dr. *Prieſtley*, in which he filled a glaſs tube with the nitrous acid

* Gaub. Phyſiol.

vapour,

vapour, and on approaching one end of it to the fire, the vapour became in that end intenſely red, while that contained in the reſt of the tube continued of a much lighter colour *; and what is not a little remarkable, the change of colour was altogether independent of gravity, or condenſation; for, whether it was held upwards or downwards, the part that was hotteſt was always reddeſt; the redneſs here was the proper effect of heat on the phlogiſton of the vapour. The caſe is nearly the ſame with reſpect to the ignition of metallic bodies, which, without receiving any phlogiſton *ab extra*, become intenſely red merely by friction ſufficient to excite into action what they contain of that principle. It may therefore be very juſtly concluded that the colouring matter of the vital fluid, as well as that of the bile is really phlogiſton, though this principle in the former may be in a ſtate ſomewhat more looſe and ſeparable than in the latter, which is a fluid much more elaborate.

There is therefore, in my opinion, a ſcale of fluids, the chyle, the blood, and the bile, which are all ſimilar, yet all different. It is

* Obſerv. and Exper. on Air.

their common character to contain a coagulable matter combined with another, which has phlogiston for its chief ingredient. In the chyle this principle is so fettered by its union with other matters, as to bear very nearly the form of vegetable expressed oils; and it seems to be gradually evolved by the action of the vessels, and at length in the blood gives evident indications of its being present in a more simple and active state. The chyle, when first taken up by the lacteals, is a bland white fluid: on mixing with the general mass of blood, it acquires a light florid colour; but it has scarcely undergone, for a few revolutions, the action of the circulating powers, when it deepens into a dark red. This, I apprehend, is a strong proof that the colour of the blood depends upon phlogiston; and the gradual deepening of it is an evident indication of a further evolution of that principle. On what other grounds can we with so much simplicity and probability account for the progressive change of colour? Why call in the aid of matters, the very existence of which in the animal fluids is universally acknowledged to be doubtful and obscure? Nothing, however, affords a stronger evidence of the principle of inflammability

mability being evolved by the action of the ſanguiferous ſyſtem, than the phænomena of reſpiration.

Reſpiration is a function ſo immediately connected with life, that no animal furniſhed with lungs, after it has once breathed, can ſubſiſt for any length of time without it. It conſiſts in an alternate dilatation and contraction of the thorax, by which air is alternately admitted and expelled from the lungs. Reſpiration in general takes place without any influence of the ſentient principle, and is as ſpontaneous as the action of the heart and inteſtines; but on particular occaſions, when for want of a freſh ſupply of air an uneaſy ſenſation is occaſioned, we are then conſcious of a propenſity to bring the organs of inſpiration into play.

What uſe reſpiration is of to the animal economy; and what change the fluids undergo in paſſing through the lungs; or, by what means the ſuppoſed changes are produced, are queſtions, which in all ages have exerciſed the ingenuity of naturaliſts, and ſtill remain ſubjects of debate in the ſchools of phyſic. All indeed agree that the alternate diſtention and collapſing of the lungs are neceſſary to expedite, and even to effect the trajection of

the blood through them. But, as *Haller* very justly observes, the use of respiration is different from this necessity, which nature might have avoided, either by using no lungs at all, or else by disposing them in a manner resembling those of the fœtus. The use, therefore, of respiration must be very considerable, since, as we formerly shewed, all animals are either provided with lungs, gills, or some similar mechanism, by which air is alternately admitted and expelled. A thousand various hypotheses have been offered to explain this matter, the chief of which I shall briefly mention, and refer the reader to Dr. *Haller*'s very learned work for a full detail of the rest *.

Hippocrates seems to have accounted air a species of aliment absolutely necessary to the life of breathing animals ‡. The opinion, however, which seems to have prevailed longest and most universally among the ancients was, that respiration is chiefly intended to temper the blood, and carry off the fuliginous vapours, with which it was loaded by the vital fire constantly kept up in the

* Elementa Physiolog.

‡ Περι φυσων.

heart.

heart. This hypothesis was strenuously supported by *Galen*, and revived by *Descartes*. *Borrelli* reckons the great use of respiration to consist in the admission and mixture of air with the blood, in order to form those elastic globules, of which it is composed; to give it its florid redness, and to prepare it for many purposes of the economy. Several later physiologists have maintained that air itself, or at least something extracted from it, a nitro-aerial matter, is by inspiration conveyed into the mass of blood, by which its elasticity is increased, and its fluidity, intestine motion, and heat, preserved. * Some ascribe the animal spirits and muscular motion to certain ethereal and active particles taken in along with the air. Others, who deny that the animal spirits are generated from the air, admit that some *pabulum vitæ*, or vital principle, is derived from it. *Malpighi* supposes this principle to be a saline vapour; *Lister*, a hot inflammable sulphureous spirit; and *Bryan Robinson*, the universal aerial acid ‡.

* Senac Traité du Cœur.

‡ De Œcon. Animal.

It appears from various passages in Dr. *Hales*'s statical experiments, that he believed that the red globules acquire considerable degrees of elastick vibrations, if not an electrical virtue, in passing through the lungs; for, says he, while by the extraordinary frictions they undergo, they are much heated and dilated, they are at the same time refrigerated and contracted by the fresh air that is continually taken into the lungs; for the coats of the vesicles are so extremely thin, that those two fluids are supposed to be one thousandth part of an inch within contact of each other, so that like blended liquors, they must needs have a considerable effect upon one another; the air in cooling the blood, and the blood in warming the air *. Dr. *Hales* is likewise of opinion, that the red colour of the globules intimates their abounding with sulphur: that the air is rendered alkaline by breathing, and that suffocation is owing to the collapsing of the minute vesicles of the lungs, in consequence of the confined air being deprived of elasticity, and the contraction occasioned by the stimulating sulphureous vapour.

* Hæmastaticks, Vol. II. *p.* 97. & passim.

Haller,

Haller, from comparing the blood of an adult perſon with that of a fœtus, and alſo with the ſame vital fluid in fiſhes, imagines he has diſcovered the final purpoſe of reſpiration in man, and other analogous animals. In the human fœtus the blood is deſtitute of its florid redneſs, and ſolid denſity; and in fiſh it has neither heat nor denſity; "and "therefore (ſays he) all thoſe properties, par-"ticularly heat, we are, by the nature of "things, perſuaded, the blood acquires in "the lungs." In corroboration of this opinion, he aſſures us that all animals furniſhed with lungs, have their blood commonly twice as warm as the circumambient air; and this he aſcribes to the violent attrition and preſſure to which the blood is ſubjected in paſſing through the lungs *. Theſe mechanical powers, however, do not in fact take place, nor are their ſuppoſed effects conſiſtent with ſound philoſophy.

A late ingenious writer, Mr. *Cigna* of *Turin*, endeavours to ſhow that air, which has been once inſpired, becomes unfit for farther reſpiration, in conſequence of noxious vapours, with which it is loaded; and ſup-

* Elem. Phyſiol.

poſes

poſes that it kills animals, in the ſame manner as all mephitick vapours, by exciting a convulſion in the lungs, and thereby impeding the trajection of the blood thro' them *. It is ſomewhat remarkable that Mr. *Cigna*, after evincing by the moſt deciſive experiments that the florid colour of the blood is cauſed by the contact of the air, ſhould, in a ſubſequent memoir, ſeem inclined to deſert that opinion, and aſcribe this effect to the evaporation of the watery parts of the blood in its paſſage through the pulmonary ſyſtem. It was generally believed that the florid colour at the ſurface of blood expoſed to the air, and the dark colour of the lower *laminæ*, was occaſioned by the different proportion of the red particles on the ſurface, and at the bottom. That opinion, however, though taught by men of the firſt abilities ||, is by no means probable; for the fibrous part, or gluten of the craſſamentum, coagulates by much too firmly to allow bodies, even heavier than the red globules, to gravitate through it. But what ſets the matter beyond all doubt, is, that Mr. *Cigna*, Mr. *Hewſon*, and Dr. *Prieſtley*

* Miſcel. Turin. Vol. I.

|| Dr. *Black*.

have

have, by the moſt concluſive experiments, ſhewn that the air has a power of changing the colour of the blood. All theſe gentlemen found that by inverting the craſſamentum, the lower laminæ, which were before black, on coming into contact with the air, conſtantly aſſumed a florid colour. The nature and cauſe of that change, I ſhall afterwards have an opportunity of explaining at ſome length. With reſpect to the uſe of reſpiration, Mr. *Cigna* ſeems inclined to think that as ſuch lungs as thoſe of man are given to the warmer animals only, the grand and ultimate purpoſe of reſpiration is to promote an exhalation from the lungs, and thereby to refrigerate the blood.

It will not be neceſſary to enter on a critical diſcuſſion of the merits of the ſeveral opinions above-recited; it being ſufficient for my purpoſe to obſerve, as every unbiaſſed phyſiologiſt muſt, that there is not one of them, which affords a ſatisfactory explanation of the phænomena, or purpoſes of reſpiration; and therefore we will dwell no longer upon them, but immediately proceed to offer ſuch a ſolution of this important phyſiological problem, as ſhall, it is hoped, be found more conſiſtent with facts and experiments.

Philosophers agree universally that a constant renewal of air is no less necessary to respiration, than to inflammation; and that in both cases, the air undergoes a change, by which, it is rendered utterly unfit for either. But in what particular manner air contributes to support the life of breathing animals, and on what account, after being frequently respired, it becomes sooner fatal to them, than an absolute *vacuum*, is a question, which, though the subject of much speculation and enquiry among naturalists, continued till lately to elude every investigation. It is now, however, proved to demonstration that common air by contributing either to the inflammation of combustible bodies, or by passing through the lungs of animals, is converted into that particular kind of air, which has been generally denominated *fixed*, or *mephitic*; but may, with equal propriety, and less ambiguity, be called *phlogisticated fixable air*, for it shall not only be shewn that it contains phlogiston, but likewise that in a separate state it is an active elastic fluid, though of such a nature, as by certain processes to be reducible to a state of fixity, attraction, and inelasticity in bodies. Dr. *Black* has an indisputable claim to the honour of the chief disco-

discoveries respecting the nature and properties of this species of air. That ingenious and modest philosopher not only discovered that air exists in a state of fixity and attraction in alkaline salts, chalk, and all calcarious substances, and that on the presence or absence of fixed air, their mild or caustic state depends, but was also from experiments led to conclude that air, in which animals have breathed, which has conduced to the burning of fuel, and that which arises from fermenting liquors, putrefying animal and vegetable bodies, as well as the noxious air of subterraneous caverns, are extremely similar in their effects, if not in every respect the same *.

Many ingenious men, in different parts of the world, have of late devoted much time and labour to the farther prosecution of this subject. In *Britain*, the Honourable Mr. *Cavendish* and Dr. *Priestley*; in *France*, *Lavoisier*; and in *Italy*, *Fontana* and *Landriani*, have already distinguished themselves, and are at present, as well as many others, engaged in the same pursuits: And indeed from the rapid progress of this branch of

* Lectures on Chymistry.

ſcience, of which nothing, in a manner, was known till very lately, there is every reaſon to believe that it will ſoon be farther advanced than any other, in the whole compaſs of natural philoſophy; and as it is an enquiry, which tends to throw light on the moſt general principles of natural knowledge, we may ſafely with *Fontana* preſage, " che già ci " avviciniamo ad una di quelle grandi epoche, " che la natura conduce, dopo un laſſo di " ſecoli, e che marca con qualche grande ſcopertà per la felicità del genere humano *." Though many experiments on different kinds of air tend to throw light on the ſubject of this eſſay, I ſhall confine myſelf entirely to the inveſtigation of the changes, which atmoſpherical air ſuffers in paſſing through the lungs of living animals.

The death of animals, and the extinction of flame are conſtantly obſerved to take place in the ſame circumſtances; and from that we are naturally led to conclude, that, though effects ſeemingly different, they both proceed from one and the ſame cauſe, and are therefore to be explained on the ſame principles. Numberleſs experiments have taught us that

* Recerche fiſiche, *&c.*

inflam-

inflammable subſtances are ſuch only, as in a certain degree of heat have a leſs attraction for the principle of inflammability, which they contain, than the ſurrounding medium has for it; whence it follows, that as ſoon as the air is fully ſaturated with phlogiſton, and incapable of carrying off any more in the ſame circumſtances, an entire ſtop is neceſſarily put to the farther ſeparation of that principle, and the flame in conſequence extinguiſhed. The caſe is perfectly the ſame with reſpect to breathing animals; for the atmoſpherical air no ſooner becomes ſaturated with the phlogiſticated matter exhaling from the lungs and ſurface of the body, than the evolved phlogiſton neceſſary accumulates in the pulmonary ſyſtem, where it muſt ſoon produce deleterious and even fatal effects.

It clearly appears from this plain and rational account of the matter, that the death of animals in air contaminated by reſpiration, or any other phlogiſtic proceſs, is not to be attributed to the want of a *pabulum vitæ*, as has been generally ſuppoſed, but to the air being overcharged with the principle of inflammability, which we have reaſon to believe acts immediately on the nervous ſyſtem; as all animals that do not preſently expire in phlogiſ-

phlogisticated air, die in violent convulsions. *Fontana* from a number of experiments concludes, that it destroys animal life in the same manner as the electrick shock *; and all experimentalists know how violently convulsed animals are on that occasion, when not instantaneously killed.

An infinitude of other facts might here be mentioned to show that the extinction of flame and animal life in the same circumstances, does not arise from a want of *pabulum ignis*, or *vitæ*, in the air, but from its being too much loaded with inflammable matter to carry off that which is perpetually separating from burning fuel, and the lungs of breathing animals. But to throw still more light on the subject in question, I shall endeavour to show that breathing animals, and flame, are not only destroyed by the same cause, but also that they both produce the same identical effects upon common air.

It appears from the latest and most accurate experiments, that by whatever process atmospherical air becomes impregnated with the phlogistic fluid, it suffers a sensible diminution, or contraction of its bulk. That,

* Recerche fisiche e filosofiche, &c.

how-

however, varies considerably, according to the manner in which it becomes mephitic; but if nitrous air be that accurate test of the salubrity of air, which it is generally accounted, we may conclude that air is more or less diminished in proportion to the quantity of the inflammable principle with which it is fraught; for we find on the admixture of nitrous air, that pure inflammable air is of all others the least contracted, and that dephlogisticated air is the most so. With respect to the comparative diminution of the quantity of air, in which a candle has burned out, and an animal expired, it has not yet been ascertained with absolute certainty. In the first instance, the diminution of air has been computed by Dr. *Hales* to be only one fifteenth, and by Dr. *Priestley* one sixteenth part, of the whole quantity; in the second, the diminution produced, has been found to be nearly equal to one fifth of the air respired. Hence, we perceive the reason why animals live for a considerable time in air, in which flame is immediately extinguished. It is however to be observed, that though air be incapable of promoting inflammation, when it has only suffered an inconsiderable diminution, yet it may, by means of combustion

properly

properly promoted, be diminiſhed as much as by reſpiration. For my purpoſe, it is ſufficient to have ſhown that in both caſes, a real diminution of the air takes place.

Another very material circumſtance, in which air vitiated by reſpiration, and air that has conduced to the burning of fuel, ſeem to agree, is that both precipitate lime from lime water. It is now well known that the ſolubility, as well as cauſticity of calcarious earths, depends on their being deprived of that ſpecies of air which is now univerſally diſtinguiſhed by the name of *fixed.* Dr. *Black* ſeems to conſider fixed air as a particular modification of common air with the principle of inflammability. Dr. *Prieſtley,* on the contrary, maintains that it is a ſpecies of air *ſui generis,* entirely different from every kind of phlogiſticated air; but the following conſiderations will, I think, make it appear that the diſpute is rather about words than things.

All who have attentively conſidered the nature of calcarious bodies agree, unanimouſly, that quick lime can only be precipitated by attracting ſuch a quantity of fixed air, as is ſufficient to reſtore it to its mild ſtate; by which means, it is no longer ſoluble in water.

Now

Now it appears from ſeveral of Dr. *Prieſtley's* own experiments, that that effect is not only produced by what he denominates fixed air, but likewiſe by almoſt every ſpecies of air into the compoſition of which the principle of inflammability enters, as a chief ingredient. Thus air vitiated by flame or reſpiration, tainted by the putrefaction of animal and vegetable ſubſtances; phlogiſticated by the fumes of charcoal, the electric ſpark, and even nitrous air, render lime-water turbid; and if there be no precipitation when brimſtone is burnt over lime water, we muſt attribute it to the calcarious matter uniting with the vitriolic acid, and forming a ſelenitick ſalt ſoluble in water.

Dr. *Prieſtley*, whilſt he admits all theſe facts, alleges that either thoſe proceſſes diſpoſe common air to depoſite the fixed air which it contains, or that there is always a quantity of it precipitated from phlogiſticated air, which attaches itſelf to the lime. That, however is a mere conjecture, and is by no means neceſſary to explain the phenomenon. The Doctor, in different parts of his experiments and obſervations, grants that fixed air contains common air, and even allows that a ſmall portion of the phlogiſtic principle

enters into its composition. Is it not then more reasonable to imagine that instead of being an acid *sui generis*, as he and Mr. *Bewley* * think, it is really no more than phlogiston modified and combined in a particular manner with the atmospherical air, and slightly fraught with the earthy gravitating particles of the bodies from which it is generated? That this is really the case, appears from its being copiously produced from metallic bodies which contain no acid. There are many phænomena which countenance this opinion, and shew that phlogiston is of an acid nature, or at least, produces similar effects. The electrical aura, which seems to be the principle of inflammability in its purest form, being made to pass through a quantity of fixed air included in a glass tube, in which also is contained water tinged blue with turnsole, litmus, or cyanus, is known to diminish the air very considerably, and change the colour of the fluid to a red, the same as if one of the mineral acids were added.

My notion of the nature of fixed air is farther corroborated by this circumstance,

* Append. vol. II. Exper. on Air.

that,

that in the calcination of metals, and other phlogiſtic proceſſes, the air, which riſes in the firſt part of the operation is generally highly inflammable, and continues in ſome meaſure to be ſuch, till the calcination is nearly compleated; when by the force of the fire, fixed air is copiouſly produced, becoming gradually more and more pure, till the whole calx, on ſome occaſions, entirely diſappears; whence, it may, with much probability, be inferred, that the fixed air in this caſe, is in fact a portion of the earthy part of the metal, combined with common air, and rendered volatile by a ſmall quantity of the principle of inflammability adhering to it. Dr. *Prieſtley* himſelf repeatedly inſinuates, that ſome portion of the phlogiſtic fluid may be neceſſary to the volatility of all bodies whatever, and that it may not be poſſible to ſeparate it entirely from any ſubſtance in nature. It appears likewiſe from his experiments, that little more than one half of the air produced on burning pounded limeſtone in a gun-barrel was *fixed*, the reſt being pure inflammable air *. From this we are

* *N. B.* Though it is probable that ſome part of this inflammable air came from the iron of the gun-barrel, we

are induced to conclude, that calcarious substances contain a large proportion of phlogiston; and, after that is once granted, how is it possible, if fixed air contained none of that principle, that it should restore quick-lime to the state in which it was before calcination? For is it not both repugnant to analogy and common sense, to pretend that a body can be restored to its original state without the reddition of all the constituent parts of which it had been deprived? These arguments render my general notion of fixed air sufficiently probable, especially if we reflect that it may be produced in considerable quantities, by means of the nitrous acid, from spirit of wine and other substances perfectly destitute of it; but which contain a pretty large proportion of the principle of inflammability.

we cannot doubt that calcarious earths contain a considerable portion of phlogiston. This sufficiently appears from their being capable of reducing, by means of heat, metallic calxes, especially those of lead and bismuth, which are easily revived, and even, in some degree, those of copper and iron. The same fact is further corroborated by their becoming true fluxes, and facilitating the fusion and vitrification of other substances, as sands and clays.

Having

Having now established on the solid basis of experiment that air, by passing through the lungs of living animals, is affected in the same manner as in every other phlogistic process; that is, diminished in its quantity, lessened in its specific gravity, and rendered unfit either for respiration or inflammation, we are induced to infer that the sole and ultimate purpose of respiration is to carry off from the body the phlogiston, which the circulating powers are perpetually evolving from the general mass of blood, and reducing to an active state. The remarkable difference between the colour of the venal and arterial blood tends strongly to elucidate and confirm this opinion. The late ingenious Mr. *Hewson* imagined, that as the colour of the blood is changed by air out of the body, the air in the lungs must be the immediate cause of the same change in the body *; and indeed several remarkable experiments of that accurate anatomist leave no room to doubt that the change above-mentioned does really take place in the lungs. On opening the thorax of living animals, he distinctly saw the blood of a more florid colour in the left auricle of

* Exper. Enquir. into the Propert. of the Blood.

the

the heart than it was in the right: but in what manner the air effects this change, he does not attempt to explain. It is however a fact, which admits of a ready ſolution, on the principles that I wiſh to eſtabliſh; and proves, in a ſatisfactory manner, not only that the blood contains an evolved phlogiſton, to which it owes its colour, but likewiſe that the air has an affinity to that principle equal to the attracting and carrying of it off, wherever they happen to come within the ſphere of activity of each other.

Various phænomena reſpecting the blood may be explained on the ſame principles. Microſcopical obſervations have taught us that it loſes the florid colour in a gradual and progreſſive manner; particularly in paſſing from the arteries to the veins in the extreme parts. That progreſſive deepening of colour can only, I think, be imputed to the principle of inflammability being gradually evolved in the courſe of circulation by the action of the veſſels; and we have the more reaſon to believe this to be the caſe, as the redneſs is more or leſs deep according to the ſtate of health which the animal at the time enjoys. Every practitioner in medicine muſt have obſerved that the blood of the vigorous and

athletick

athletick is of a rich deep crimson; that of the relaxed, leuco-phlegmatic and dropsical, of a pale red colour. These are striking facts, and may justly be accounted conclusive proofs that the action of the vascular system really evolves the phlogistic fluid, and separates it, in an active state, from the circulating mass.

Dr. *Priestley*, in the last volume of his experiments and observations on different kinds of air, has inserted a paper on respiration, and the use of the blood; in which, it gives me pleasure to find, that his sentiments in several respects entirely coincide with those I had published two years before, on the same subject *. The Doctor, from discovering that air, which has passed through the lungs is left in precisely the same state as that, which has been exposed to the most obvious cause of phlogistication, concludes that respiration is a true phlogistic process; and because all the blood in the body passes thro' the lungs, and that the change betwixt the colour of the venal and arterial blood takes place there, he thinks, that it is by means of the blood that the air becomes phlogisticated in its trajec-

* Dissert. Phys. de Cal. Animal. Causa. *Edin.* 1775.

tion

tion through that organ: "And therefore, " (pursues he) one great use of the blood " must be to discharge the phlogiston, with " which the animal system abounds, im- " bibing it in the course of its circulation, " and imparting it to the air, with which it " is nearly brought into contact, in the lungs, " the air thus acting as the great menstruum " for this purpose." These are Dr. *Priestley*'s words, and though I do not in the least question his knowledge in the art of logick, I must be permitted to say that, on this occasion, he has, from the justest principles deduced the most erroneous conclusion.

The two first propositions appear to me indisputably true; but I must confess myself astonished that a man of Dr. *Priestley*'s knowledge and judgment, should betray so much ignorance of the animal economy as to suppose that the blood *imbibes* phlogiston in the course of its circulation. The most superficial view of the process of sanguification would have prevented him from falling into so palpable an errour. Had he only reflected that nothing can be taken into the animal body, or become a component part of its fluids, but what is either formally or materially present in the aliment before its assumption

aſſumption into the ſtomach; that there, and in the courſe of the chylo-poietic *viſcera*, by the proceſs of digeſtion is ſeparated under the form of an emulſion, whatever is afterwards to be aſſimilated to the animal body; and that that fluid, which phyſiologiſts have denominated *chyle*, is taken up by millions of ſmall abſorbent veſſels; which, from imperceptible beginnings on the internal ſurface of the inteſtines, unite into larger veſſels, and convey the chyle, firſt into the conglobate glands of the meſentery, and thence to the receptacle, from which it paſſes by the thoracic duct into the left ſubclavian vein, where it mixes with the common maſs of blood, and after a few revolutions becomes a proper ſubject of nutrition to the frame, and of ſupply to the conſtant waſte, which every moment of our lives it muſt neceſſarily ſuſtain. Had Dr. *Prieſtley*, I ſay, adverted to theſe obvious conſiderations, inſtead of ſuppoſing that one great uſe of the blood is to *imbibe* phlogiſton from the ſyſtem, he muſt on the contrary have perceived that it is the only channel through which the animal economy can be ſupplied, in a permanent manner, with that principle.

It is therefore plain, that though Dr. *Prieſtley* was right in fancying that phlogiſton is carried off from the blood in the action of reſpiration, yet his deductions with reſpect to the uſe of that fluid are repugnant to facts, and every principle of ſound reaſoning. Had he ſaid that the chief uſe of reſpiration is to diſcharge into the common menſtruum, the atmoſphere, the phlogiſton which is evolved from the blood by the action of the circulating powers, he would have drawn a more juſt concluſion, and ſuch as his own ingenious experiments ſufficiently warrant.

Though a recapitulation, or collective view of the ſeveral facts which have been brought to ſhew that phlogiſton is really evolved from the common maſs of blood by the action of the circulating powers, would doubtleſs ſet the matter in the cleareſt point of light, yet to avoid prolixity, I ſhall only beg leave to remind the attentive reader, that phlogiſton is the chief ingredient in all alimentary ſubſtances; that a chymical analyſis evinces its preſence in the blood; that it is the principle of taſte and colouring matter in the bile; that the chyle, which when taken up by the lacteals is a bland white fluid, after being ſubjected, for a ſhort time, to the action of the

veſſels,

veſſels, becomes of a deep red colour; that the *halitus*, which eſcapes from blood recently drawn, the vapour flying off from every part of the body, and particularly from the lungs, conſiſt chiefly of the principle of inflammability: If the reader, I ſay, will only reflect on theſe ſeveral facts, and allowing each of them its due weight, draw juſt concluſions from them, he will hardly, I think, entertain any doubt of the truth of the propoſition which they are meant to eſtabliſh: for we ought not, ſays Mr. *Locke*, "peremptorily, or intemperately, to require demonſtration, and demand certainty, where probability only is to be had." He that will not eat till he has demonſtration that it will nouriſh him, may ſit ſtill and periſh.

Senſe and *intuition* reach but a ſhort way. The greateſt part of our knowledge depends upon deductions and intermediate ideas. In thoſe caſes, therefore, in which demonſtrative proofs and certain knowledge are not to be had, we muſt content ourſelves with probabilities. And hence before we can rationally admit, or reject, any propoſition, we ought to find out, examine, and maturely weigh all the grounds of probability, and upon a due balancing of the whole, aſſent,

or dissent, according to the preponderancy of the arguments, for or against the proposition in question. That is the test to which new theorems ought to be put, and the only mode by which, we can with impartiality and candour estimate the credit they deserve. Instead, however, of pursuing that truly just and rational method of investigating truth, we are but too apt, when any proposition is advanced, to consider whether it tallies with our own prior sentiments, not whether it be true or false; and consequently not judging of it by its evidence, but by those notions we have already imbibed, according to its agreement, or disagreement with those, we admit or reject it. This procedure is inconsistent with true philosophy, and evidently one of the greatest obstacles to the spreading of useful knowledge, and receiving of information from the labours of others. We should, therefore, to give to an argument urged by any one its due weight, lay aside all we think ourselves on the subject, and confine our views entirely to what is before us; and when we have thus, without prejudice, or partiality, considered the grounds on which he builds his opinion, we may call over again every particular, try it by our own *præcognita*, set

fet the old evidence againſt the new, and after giving it a fair trial, either embrace or reject it as we ſhall find it ſupported by facts, and ſound reaſoning. Novel opinions, whether in ethics, or phyſics, have a thouſand difficulties to combat, and none more formidable than prejudice. It is for this reaſon, that I hope the reader will for a time loſe ſight of his own opinion, and conſider and view things in the light I have put them, without rejecting what is advanced, merely becauſe it is not the ſame, or may perhaps contradict what he before believed.

But to return to my ſubject. Though it may be preſumed that the evolution of phlogiſton from the vital fluid has been already rendered probable, yet that the ſame fact may be farther illuſtrated and confirmed, we ſhall briefly inveſtigate the nature of alimentary ſubſtances. The ingenious M. *Buffon* in explaining the nature of the nutritious matter, which animals aſſimilate to their own ſubſtance, ſuppoſes that nature abounds with an infinity of living organized parts, the production of which coſt her nothing, becauſe their exiſtence is conſtant and invariable; as thoſe cauſes which ſeem to deſtroy them, do in fact, no more than ſeparate them. In the aliment,

aliment, ſays he, which animals take to ſupport the vital powers, there is a large proportion of organical *molecules*, or parts analogous to animated body, which are by the animal functions ſeparated from the inert, and paſſive particles of matter; and whilſt theſe are thrown off, as recrementitious, by the different emunctories, thoſe are retained to become the immediate nutriment of the parts, and to ſupply the conſtant waſte *. The grand argument on which M. *Buffon* ſeems to found his opinion, is, that the whole animal creation ſubſiſts either on animal or vegetable matter, the conſtituent parts of which he thinks himſelf warranted to conſider as organical molecules.

This hypotheſis is to be ſure extremely plauſible, and carries with it ſtrong marks of that ingenuity and warmth of imagination, which ſtamp every page of the writings of that celebrated naturaliſt; but on a cloſe examination, it will not be found eſtabliſhed on facts ſufficiently obvious and concluſive to afford conviction. What ſolid reaſon have we for fancying that nature ſhould *ab origine* have created two diſtinct kinds of matter,

* Hiſtoire Naturelle. *Tom.* III.

one

one for organized, and another for unorganized bodies? What chymical experiment has ever ſhewn any peculiar principle in the compoſition of animal and vegetable ſubſtances, which is not to be found in the moſt inert and lifeleſs maſs of matter? In fine, how are we to conceive that thoſe living organical molecules, as M. *Buffon* aſſerts, are by fire, putrefaction, and the other efficient cauſes of the total diſſolution of organized bodies, only ſeparated and not aboliſhed? That the ultimate parts or atoms of bodies are immutable cannot I think be rationally queſtioned; but wherever there is life or organization, there muſt be a peculiar mechaniſm dependant on the mode of combination, the arrangement and actual juxta-poſition of the component parts. Hence, whatever ſeparates thoſe parts, produces a change of form, and though they be not entirely aboliſhed or annihilated, yet ſince the peculiar arrangement, to which the life and organization of the compound was unqueſtionably owing, is on ſuch occaſions irrecoverably deſtroyed, what probable ground is there for fancying that thoſe particles of matter, tho' they formerly were conſtituents of an organized body, ſhould now differ in any reſpect

from

from thoſe, that enter into the compoſition of inert ſubſtances? For animal life, in contradiſtinction to conſciouſneſs, and thoſe higher powers of the rational ſoul, which we believe to have an exiſtence independent of matter and mechaniſm, ſignifies no more than a ſyſtem of materials combined and arranged in ſuch a manner as to conſtitute a machine, from the mechaniſm of which reſults a regular and conſtant ſeries of thoſe motions and operations, which phyſiologiſts have denominated natural, vital, and animal functions *; and with reſpect to vegetable life,

* Whilſt I thus endeavour to ſhow the extenſive influence of material mechaniſm on the functions of animated body, I, at the ſame time, utterly reprobate the opinion of a late ingenious writer, who has laboured to prove that man is *no more* than what we ſee of him: A doctrine of a much more dangerous tendency than that ſubtle metaphyſician ſeems aware of: For while he glories in having wiped off the *reproach* of matter, he not only degrades man to a mere automaton, but leads to opinions highly derogatory to the attributes of the Deity. If ſpirit, as Dr. *Prieſtley* maintains, be incapable of acting upon matter, the divine eſſence muſt be material, and whatever is material, muſt, by the ſame rule, be regulated by the laws of material mechaniſm. Hence, though the notion that GOD is a free, immaterial and omnipreſent being, and that man is endowed with an immaterial

life, naturalists can certainly mean no more by it than an arrangement of certain materials, from which, results a particular organization of parts destitute of sense and spontaneous loco-motion, but capable of accretion and generation.

Since therefore both animal and vegetable life depend on material mechanism, is it not absurd to assert that though the mechanical arrangement of parts, to which a body owed its organization be abolished, the severed parts still continue to be organick. With equal justice might an absurd logician maintain that, as syllables consist of letters, words of syllables, verses of words, and an epick poem of these, letters are poetic parts of the *Iliad*; for supposing all the verses of that divine poem subdivided into words, syllables and letters, these would have as much right

immaterial thinking principle, be considered by that theologue as contrary to the acknowledged rules of philosophizing, yet I am not ashamed to confess myself so contemptible a metaphysician, and so tenacious of vulgar prejudices, as to think that it would redound more to the honour of the Supreme Being, to the dignity of human nature, and the advancement of useful knowledge, that he devoted his time and talents to less speculative and mysterious disquisitions.

to be ſtyled *poetic*, as the decompounded conſtituents of an organized body to be called *organic*. But an argument entirely ſubverſive of M. *Buffon*'s hypotheſis is, that by chymical combination we can not only make the chief ingredients of organized bodies paſs into the compoſition of inert matter, but have even every reaſon to believe that the latter may, by the aſſimilating powers inherent in animal and vegetable bodies, be tranſmuted into their own organic ſubſtance. With reſpect to the nature of the individual matter, which is capable of that aſſimilation, it is ſtill a *deſideratum* in natural hiſtory; but from a multiplicity of concurring circumſtances I am induced to account phlogiſton the immediate principle of animal and vegetable nutriment. That this is the caſe with reſpect ro every ſpecies of plant, I ſhall afterwards have occaſion to evince by the moſt concluſive proofs; and the ſubſequent obſervations will, I think, render it highly probable that animal bodies derive all their ſupport from the ſame ſource.

If we ſurvey the whole animal kingdom, and inveſtigate the nature of the aliment on which its numberleſs tribes ſubſiſt, we ſhall

find

find that no ſubſtance in nature affords any one of them matter capable of aſſimilation, unleſs it contain the phlogiſtic fluid, and contain it in ſuch a ſtate as to be readily ſeparated by the animal functions. Man, from obſervation, and the brute creation, from unerring inſtinct, are led to believe that alimentary ſubſtances afford more or leſs nouriſhment in proportion to the quantity of phlogiſton, which they poſſeſs in a looſe and ſeparable condition. I ſhall appeal to any one alderman in the city, whoſe digeſtive powers have not been lately impaired by frequent ſurfeits, if he does not find leſs ſolid nouriſhment in vegetable than animal food; in chicken, than in a ſirloin of beef, or haunch of veniſon; in lobſter or whiting, than in turtle, ſalmon, or eel: yet, all theſe are organized bodies, the only circumſtance, according to M. *Buffon*'s theory, that is neceſſary for their ſupplying the living ſyſtem with nutritious matter; but on a nearer view, and cloſer examination, we find that each of the above-mentioned ſubſtances affords more or leſs nutriment, in proportion to the quantity and ſeparable ſtate of the inflammable matter, which enters into its compoſition.

Since, therefore, those substances only can be assimilated to the animal body, which possess the principle of inflammability in a certain state, and since we have seen that that principle is again discharged from the lungs and surface of the body under the form of perspirable matter, have we not every reason to believe that animals have a power of converting phlogiston from the fixed state, in which they receive it, in their aliment, into a pure and active state, and perhaps not very different from the electric fluid. By what other principles can we explain the singular phænomena of the torpedo, and the *anguille tremblante* of *Surinam?* And does not the light, which is known to proceed from some animals, as from cats and wild beasts, when they are in pursuit of their prey in the night, afford the most convincing proofs that, in consequence of the various functions and operations peculiar to animated body, phlogiston is extricated and evolved from the alimentary matters, in which it was fettered, and brought into a state, under which it exhibits certain indications of its presence and activity? *Bartholin*, in his learned treatise *De Luce Animantium*, and

and ſeveral later writers, furniſh us with numberleſs inſtances of the actual emiſſion of light from living bodies *; and that light, as well as the electrical aura, is really the phlogiſtic fluid under a particular form of action, ſhall be afterwards evinced by indiſputed facts. It may therefore be reaſonably concluded, that phlogiſton is the nutritious matter, to which the animal machine owes its accretion, vigour, and conſtant ſupport; and as the material cauſe of all muſcular motion can only be derived from the aliment, it is highly probable that the principle of inflammability, after being tranſmuted by the animal proceſſes into that ſtate, in which it is called the electric fluid, is by means of the nervous ſyſtem directed to the muſcles,

* The annals of natural hiſtory afford us many well vouched inſtances of perſons whoſe bodies emitted light in the dark. Dr. *John Fabri*, a noted philoſopher, informs us, that he ſaw flaſhes of light proceed from the head of a woman while ſhe combed her hair. *Scaliger* relates the ſame of another woman; *Euſebius* of *Maximus Acquilanus*; *Cardanus* of a friend of his, and Mr. *Brydone* of a young lady of his acquaintance; and that moſt animal ſubſtances, during the putrefactive ſtage of fermentation, emit light, is a fact too notorious to ſtand in need of proof.

and

and forces them to act in the same manner, as they are forced into action, when the electric fluid is thrown into them *ab extra* *.

* Dr. *Priestley*'s Exper. on Air. *Vol.* I.

CHAP.

CHAP. IV.

The Evolution of Phlogiston is attended with Heat.

THE doctrine of heat taught by modern philoſophers is extremely various. It is diſputed by them, whether fire be coeval with bodies and of an abſolute ingenerable nature, or merely the effect of motion. Theſe queſtions have been ſtrenuouſly debated, and two ſects of philoſophers, the Chymical and Mechanical, have ſprung from them. The mechanical philoſophers maintain that heat is nothing but a particular mode of motion, and conſequently that the heat generated in bodies does not depend upon their poſſeſſing a greater or leſs quantity of elementary fire, but on their being more or leſs capable of having vibratory motions excited in them; and this capability, they affirm, depends on a certain concretion, or form of aggregation, and is in no degree dependent on the mixture of the body *.

In

* Lord *Bacon*, in his treatiſe *De Forma Calidi* (which notwithſtanding a certain profoundity, not to ſay obſcurity

In explaining, however, this concretion, which gives elaſticity, or a power of having thoſe oſcillatory motions, in which according to them heat conſiſts, they have ſuppoſed that there are two kinds of matter, a gravitating inert matter, and a ſubtile elaſtick fluid, which pervades all bodies without affecting their mixture. The elaſticity too of bodies is aſcribed by them to the ſolid gravitating parts being embraced by this ſubtile fluid, which modifies, by the different degrees of its denſity on the ſurface, and in the pores of bodies, their concretion, and gives all the variety of aggregation of the parts of mixts *.

The

ſcurity that runs throughout may juſtly be held up as a model of philoſophical inveſtigation), defines heat thus: "Calor eſt motus expanſivus cohibitus et nitens per "partes minores: et intelligatur, *ſays he*, hoc quod "diximus de motu (nempe quod ſit inſtar generis ad "calorem) non quod calor generet motum aut quod "motus generat calorem, ſed quod ipſiſſimus calor, "ſive *quidipſum* caloris ſit *motus* & *nihil aliud*."

* It may not be improper to remark, that this ſect of philoſophers maintains that the concretion of bodies depends in no degree upon the nature of their mixture; for the concretion, ſay they, or the aggregation of the parts of mixts, may be the ſame where their mixture is

very

The chymical philoſophers on the other hand, convinced that motion is the great inſtrument of all the effects, operations and phænomena, that are produced in the material univerſe; and that matter, or bodies without it, can neither ſuffer any change in themſelves, nor produce any alteration in other bodies, readily allow that motion attends the generation of heat, but they at the ſame time maintain that heat depends on a ſimple primary ingredient of all natural bodies; and they aſſert that bodies are more or leſs fit for the production of it, according to the quantity that they contain of this principle, which, according to *Boerhaave*, is a body *ſui generis*, created ſuch *ab origine*, unalterable in its nature and properties, and not either producible *de novo* from any other body, or capable of being reduced into any other body, or of ceaſing to be fire *. This is not the vague conjecture of modern chymiſtry, but is as ancient as the ſyſtem of atoms, and was a fundamental principle laid down by the moſt celebrated

very different; and on the contrary the mode of concretion may be very different where the mixture is perfectly the ſame, as iron and ſteel; but how far this opinion is eſtabliſhed in fact, ſhall be hereafter ſhewn.

* Elem. Chym.

brated atomical philosophers of *Phœnicia* and *Greece*, to whom it had no doubt been suggested by their early observation of nature, before her plain appearances were obscured by the imaginary schemes, and idle disputes, of speculative men.

The chief supporters of the mechanical system are Lord *Bacon*, Mr. *Boyle*, and Sir *Isaac Newton*: All the foreign philosophers, *Homberg*, *Boerhaave*, *Lemery* the younger, and *S'Gravesande*, maintain the chymical. These names merit the highest deference, yet I think it may be shewn that the dispute is more about words than things. It is not a little to be regreted that in philosophy, as well as politics, we are often misled by prejudice, and a partial view of the matter in debate; and in the heat of argumentation hazard opinions, in support of our favourite system, that are not founded on facts and just reasoning. Hence it generally happens, that if we mean to settle any controverted point, large concessions, on both sides, become necessary; and this, on the present occasion, I suspect, we shall find to be the case.

In the first place, it is no less confidently asserted by the chymical philosophers, that the capability in bodies of generating heat, entirely

entirely depends upon the quantity, which they possess of elementary fire, than it is strenuously insisted on by the mechanical, that heat is in no shape dependent on that circumstance, but is owing solely to the mode of concretion. Both these assertions, however, we shall find to be too general. For on the one hand, it must be granted that a certain form of aggregation is requisite for the mechanical production of heat, and by consequence that one body may be better adapted to the excitation of heat in this way, than another, which may perhaps contain double the quantity of elementary fire: and on the other hand, it will be found that bodies in general, *cæteris paribus*, are more or less qualified for the production of heat, even in the mechanical way, according to the quantity of potential fire possessed by them.

The instance of *iron* and *steel*, which has been often brought to prove the contrary, seems to be a strong evidence in favour of this opinion. It is universally known that by the conversion of iron into steel, this metal acquires a closer, more compact and finer grained texture, a greater hardness, elasticity, sonorousness, and disposition to receive the magnetick property. The opinions

offered to explain the cause of this change are various; but on considering the methods always used to effect it, I do not hesitate to ascribe the properties, by which steel differs from iron, to a greater proportion of the principle of inflammability. Dr. *Cullen* indeed, whose name I shall always mention with the highest veneration, amongst the other arguments, which he hath used in favour of the mechanical generation of heat, maintains that the transmutation of iron into steel does not depend on the introduction of any foreign matter into the iron, but merely on the manner of its cooling. In this, however, he differs from *Stahl*, *Cramer*, *Macquer*, *Newman*, Dr. *Black*, and all the best chymists, who seem universally to agree that inflammable substances, as the horns, hair, and hoofs of animals, or mixtures of soot and charcoal, are absolutely necessary to the process in question; and if it be found that steel heated and suddenly quenched in water does in fact become harder, may not we with great reason ascribe this increase of hardness to the sudden contraction and diminution of the mass in all its dimensions, by which its parts must necessarily be brought into much closer contact? Besides, as water is a slow

conductor

conductor of electricity, and heat, and possessed of no strong attraction for phlogiston *, it must necessarily prevent such a quantity of that principle from escaping, as would have been carried off, had the mass been exposed to a free current of air. But a conclusive argument that steel contains more inflammable matter than iron is, that Dr. *Priestley* found that eleven grains of steel filings from watch-springs, yielded one-twentieth more inflammable air than the same weight of iron-filings ||.

From what has been said of the cementation of steel, it is obvious that no argument can be thence drawn against the existence of a particular matter, which enters in different proportions into the compositions of mixts, and lies latent and inactive, till by some means, or other, it be excited into motion; when, according to the nature of the substances with which it is combined, all the phænomena of light and heat are produced.

Dr. *Hales*, though a champion in defence of the mechanical doctrine of fire, is obliged to acknowledge "that fire is chiefly invigo-"rated by the action and reaction of the acid

* Obferv. and Exper. on Air. || Ibidem.

"*sulphu-*

" *sulphureous* particles of the fuel, and the
" elastic ones, which arise and enter the fire,
" either from the fuel, in which they a-
" bound, or from the circumambient air.
" But when the acid *sulphur* is taken out of
" any fuel, the remaining salt, water, and
" earth, are not inflammable, but on the
" contrary quench and retard fire; and as air
" cannot produce fire without *sulphur*, so
" neither can *sulphur* burn without air *."
Is it not apparent from the above passage of Dr. *Hales*, that there is a particular kind of matter, which alone is capable of producing fire; and that it is no other than *sulphur*, by which all the ancient chymists meant what has been since, and more properly, called *the principle of inflammability*.

In general, however, the mechanical philosophers, who ground their opinion chiefly on the production of heat by attrition and percussion, think it absurd to suppose that the heat excited in these instances, should in any manner depend on the mixture of the body, and challenge any chymist to show that bodies are more or less fit for vibratory motions according to the quantity of elemental fire,

* Statical Experiments. *Vol.* II.

which

which they poſſeſs. This argument they conſider as an unanſwerable and irrefragable proof of the ſolid baſis on which their ſyſtem is founded.

I ſhall not now repeat what was ſaid on the tranſmutation of iron into ſteel, though evidently tending to ſhow the fallacy of this aſſertion, and ſhall even admit it as a general fact that the fitneſs in bodies for generating heat depends more on their mode of concretion, than on any particular matter, which enters into their compoſition; but ſtill I maintain, that no concluſive inference can be drawn from thence, in favour of the mechanical ſyſtem: for I hope to make it appear not leſs probable, that the phlogiſtic fluid gives the concretion, than that the concretion gives the fitneſs to generate heat.

The opinions which have been advanced reſpecting the *vinculum elimentorum primarium*, or the principle, which forms the cement, or bond of union between the ſeveral conſtituent particles of bodies, are many and various. Some aſcribe coheſion ſolely to the attraction ſubſiſting between the particles of elementary earth *: Others conſider water as

* Ganbius Inſtit. Pathalog.

the

the cementing principle; but the most universally received opinion is that of the learned Dr. *Hales*, who attributes cohesion to that particular kind of air, which has been denominated fixed, or mephitick air. "Doubt-"less (says he), all the particles of matter "whatever, do in actual contact cohere; "yet, since it is found by experiments that "the most solid parts of animals and vege-"tables yield a vastly greater quantity of air, "and less water, than the more lax and fluid "parts, it seems therefore reasonable to con-"clude, that their solidity is principally ow-"ing not to the watery, but to the *air*, and "*sulphurous particles* *." The same ingenious philosopher expressly says, that "air "is very instrumental in the production and "growth of animals and vegetables, both "by invigorating their several juices while in "an *elastick active* state, and also by greatly "contributing in a *fixed* state to the *union* "and firm *connection* of the several constitu-"ent parts of those bodies ‡." This doctrine has been lately revived by Dr. *Macbride*, who has laboured to prove that fixed air is

* Statical Experiments. *Vol.* II.

‡ Ibidem. *Vol.* I.

the

the cementing principle in all animal and vegetable ſubſtances. But ſuch an hypotheſis every philoſopher will reject, as repugnant to the general procedure of nature, who never multiplies agents without an abſolute neceſſity. Her operations are ſimple, her laws general; and hence it is abſurd to ſuppoſe that any thing ſhould conſtitute the bond of union in bodies, but an unbounded and univerſal principle; a principle equally applicable to the whole range of beings in the animal, vegetable and mineral kingdoms. This however is ſo far from being the caſe with Dr. *Macbride*'s theory, that he readily acknowledges that ſaline particles may put on concrete forms without the aid of fixed air; and even owns that he failed in all his attempts to reduce metallic calces by means of that ſuppoſed principle of coheſion.

In explaining the cauſe of the augmentation of weight in metallic calcinations, I had occaſion to obſerve that though gravity ſeems chiefly to depend on an external impelling power, all bodies according to their mixture, and the quantity of matter they contain, reciprocally attract and are attracted by each other; and that from hence ariſes the tendency to the centre of the larger ſyſtems

of matter. I ſhall now venture a ſtep farther, and endeavour to ſhow that the ſame material agent which produces a mutual attraction betwixt large bodies, produces the ſame effect between the minute corpuſcles of matter; in a word, that the cauſe of attraction and coheſion is one and the ſame.

Gravitation and attraction, though generally confounded by philoſophers, to me appear very different phænomena. Attraction is peculiar to the ſmalleſt particles of matter, and is powerfully exerted between them; its ſphere of activity is greater in ſome atoms than in others; and though hardly diſcernible in any, unleſs when they are in immediate contact, or verging upon it, yet when they approach one another they acquire an obſtinate coheſion. Gravitation, on the contrary, is common to all bodies, and though it brings them together with great violence, no coheſion at all is produced; as appears in the caſe of falling bodies. Since therefore gravitation affects indiſcriminately all bodies, at all diſtances, and in proportion to the quantity of matter they contain; whereas attraction affects only the primigenial particles of matter, is exerted at ſmall diſtances only, and that too with more or leſs force according

according to the nature of the compound, we are naturally led to conclude that the former is the effect of an external impelling power, while the latter depends on a particular ingredient present in different proportions in all terrestrial bodies. Were attraction a general law of matter, all bodies should attract each other in proportion to their masses. Hence, the loadstone ought to have as little of that virtue as the emerald, and be as little inclined to attract iron as lead. Upon the whole, it is plain that two phænomena, so widely different as gravity and attraction, cannot be produced by the same means. How then are we to explain them without having recourse to some new mechanical agent? Not, I acknowledge, without some difficulty; but it may, I think, be done with no small degree of probability. It was formerly said, that gravitation is effected by the pressure of the universal elastick æthereal fluid, and an attempt shall now be made to show that attraction depends on the same subtile medium fixed and modified in bodies.

The most striking instances of attraction are those of electricity and magnetism. With respect to the former, I need only observe, that it is the obvious effect of exciting into

 motion

motion a certain ſubtile fluid, lodged on the ſurface and in the pores of bodies, which fluid, to every appearance, is the *æther* of *Newton*, and the *phlogiſton* of the chymiſts. The cauſe of magnetic attraction is more obſcure; and has been long a noted problem in phyſics. But the veil in which that dark myſterious queſtion was ſo long inwrapped, is now removed. For many late accurate experiments have not only ſhown, that polarity may, with as great certainty, be communicated to ſteel by electricity as by the magnet, but likewiſe that it is entirely aboliſhed by calcination*: an irrefragable proof that the attraction of electricity and magnetiſm are owing to the ſame material cauſe, the ethereal phlogiſtic fluid. Chymiſtry affords us inſtances without number in which the ſame ſubtle elaſtick element is evidently the principle of attraction, and the bond of union in bodies. But as a full diſcuſſion of that curious queſtion does not fall within the limits of this ſcanty page, it will be ſufficient to obſerve that there is not a more general fact in phyſics, than that the phlogiſtic fluid in bodies is no ſooner excited into mo-

* Brugman. de Magnetiſm. &c.

tion,

tion, than the affinity of cohesion is diminished, the mass dilated and increased in all its dimensions; nay a total dissolution immediately ensues upon the escape, or dissipation of that principle. This is sufficiently manifest in the combustion of inflammable bodies, and the calcination of metals; and it is confirmed by the phænomena attending the putrefaction of vegetable and animal substances, which, when exposed to the air, gradually lose their phlogiston, grow tender, rot, and at last suffer a total dissolution of their parts. A phenomenon in every respect similar to the putrefaction of animal and vegetable substances, is the *rusting* of metals; a change, which they undergo in consequence of being exposed to the air. The nature of this change is extremely obvious; for we always find a rust to be a calx of the metal, in no wise different from that, which is produced by proper calcination. The effect, in both instances, depends on a separation of the principle of inflammability, and is in both produced by the attraction, which the air has for it. Hence we are led to comprehend why neither putrefaction, calcination, nor rusting take place *in vacuo*.

Though I by no means dispute the accuracy

racy of Dr. *Macbride's* experiments, yet his deductions from them are dubious and controvertible. He seems in the first place not to have sufficiently distinguished between what is properly denominated fixed air, and what the Hon. Mr. *Cavendish* and all modern chymists call inflammable air; two species of gas extremely different in their nature, origin, and properties. Under the denomination of fixed air, he seems to comprehend all kinds of air, that are not common atmospheric air, how different soever they may be with respect to their production, their comparative weight, density, or the quantity of phlogiston with which they are fraught. Dr. *Macbride*, by thus confounding things that ought certainly to have been accurately defined and distinguished, has left his theory open to various and innumerable objections; but it is sufficient for my purpose to show, that he has been all along misled by taking the effect for the cause. Nothing is more obvious than that the dissolution of animal and vegetable substances is attended with the generation and escape of fixed air; but it is to be observed, that this phenomenon seldom takes place till the cohesion of the constituent parts is sensibly diminished; for it is then, and not

till

till then, that the atmoſpherical air, which enters in great abundance into the compoſition of all ſubſtances of that nature being let looſe, attaches itſelf to a ſmall portion of the phlogiſtic fluid, and perhaps of ſome other of the original conſtituents of the maſs, and by this new combination, acquires thoſe properties, for which modern chymiſtry has diſtinguiſhed it, by the name of *fixed* *. This opinion is confirmed and elucidated by the air, which flies off from bodies, whether ſolid or fluid,

* Dr. *Prieſtley* found, on ſubjecting a piece of mutton to the putrefactive fermentation, that all the inflammable gas was extricated in the beginning of the proceſs. He likewiſe found that pit-coal diſtilled in a glaſs veſſel yielded only inflammable air, but that the aſhes of the ſame pit-coal yielded much air, of which one half was inflammable, the other fixed. *Vol.* III. Exper. on Air.—When a ſtrong and ſudden heat is applied to animal or vegetable matters, the elaſtic fluid produced is generally a mixture of air, which renders the cauſtic alkali mild, and of inflammable gas ſeparable from it by expoſure to water: but when the proceſs is not haſtened by fire, the eſcape of the inflammable air generally precedes that of the fixed. There is evidently then no more reaſon for conſidering fixed air as the connective inſtrument in bodies than any of the other conſtituents of mixts. That in metallic bodies at leaſt, fixed air is not the principle of coheſion, plainly appears from their containing leſs of that fluid in their malleable than calcined ſtate.

fluid, in the exhausted receiver of an air pump; possessing none of those properties that characterize mephitic air; for where no separation of phlogiston has previously taken place, no fixed air is ever observed to escape, and this is a strong presumptive proof, that it is no more than a circumstance consequent on the decomposition of natural bodies, and by no means the efficient cause of their dissolution, as Dr. *Macbride* strenuously maintains.

With respect to the nature of fixed air, Dr. *Hales*'s notion and mine pretty nearly agree; for he, as appears from several passages in his writings, considered it as a portion of the universal aerial fluid, deprived of its elasticity, and reduced to a state of fixity and attraction by the power of the *sulphur principle* in bodies. And that this is in fact the case, we had formerly an opportunity of shewing at considerable length. But Dr. *Macbride*, as has been already observed, seems to have had no distinct idea of the specific difference between fixed air and the other species of noxious gases. The vapour, which arises from some putrifying vegetable substances, as the cotton wood, when collected, proves absolutely inflammable: That, however he supposed to be fixed air; but it appears

appears from Mr. *Cavendish*'s experiments *, that it contains very little of that, but is composed of an extremely light and subtile vapour, ten times lighter than the common vital air, when this is 800 times lighter than water; whereas fixed mephitick air is 1-half heavier; and with respect to the small quantity of fixed air, which is found with it, it may either rise from the subject itself, or be produced from the above-mentioned subtile inflammable vapour, which has the power of converting common air into mephitick. The case is pretty much the same with respect to metallic bodies. The air, which first arises either during the calcination of metals by fire, or their solution in the mineral acids, is, for the most part, a pure inflammable vapour; and the decomposition is generally far advanced before any fixed air can be collected: And finally it may almost, I apprehend, be considered as an *argumentum crucis* against Dr. *Macbride*'s supposition of fixed air being the cementing principle in natural bodies, that Dr. *Priestley* has made several accurate experiments, which leave no room to doubt of its being a factitious substance. In his obser-

* Philosoph. Transf. passim.

vations and experiments on different kinds of air, he mentions a proceſs, in which, by means of the ſpirit of nitre, he produced fixed air in conſiderable quantity from ſpirit of wine, and other ſubſtances, which do not contain it: And can any thing be more diſſonant from ſound philoſophy, than to ſuppoſe that the principle of union between the component parts of mixts, ſhould be of a factitious nature?

It is, I acknowledge, not eaſy to explain how phlogiſton, which is here conſidered as the cementing principle in bodies, ſhould have the property of paſſing from a highly volatile and repellent elaſtick ſtate, to an inelaſtick and ſtrongly attracting ſtate. That this however is the caſe, appears from experiments without number, and the following ſeems the moſt plauſible explanation of which the fact admits. It was formerly alledged that the phlogiſtic fluid has a centrifugal tendency, or is at leaſt exempt from the common laws of gravitation; and to this circumſtance is owing the extreme volatility which it exhibits, both in a ſeparate and compounded ſtate. But notwithſtanding the volatility of phlogiſton, it ſhows a great affinity to many ſubſtances of an inert gravitating nature. When

When the quantity of inert matter does not preponderate over the elaſtick power of that principle, the compound becomes volatile, as in the volatile ſulphurous and nitrous acids, and a thouſand ſimilar inſtances; but when the gravitating matter is ſufficient to repreſs the volatility of phlogiſton, as in metallic bodies, this principle, though it in fact renders the mixt lighter and more elaſtick, exerts no power ſo ſenſibly as that of attraction, and this it continues to do, till it be again excited into motion by ſome external cauſe, ſuch as adventitious particles of fire, friction or mixture, when its elaſticity being reſtored, it flies off, fraught with as much of the ponderous earthy matter as it is capable of volatilizing.

From the above ſtrictures on Dr. *Macbride*'s theory of coheſion, it will not, I preſume, appear raſh to conclude that fixed air cannot poſſibly be that univerſal cementing principle, which he believes it; and that, if any ſuch matter does in fact ſubſiſt, it is unqueſtionably the principle of inflammability; and indeed, as a late ingenious writer obſerves, it was always well known to a claſs of philoſophers now out of date, that the parts of all conſiſtent bodies are bound together

by a ſecret acid principle; an embodied ether, which acts as the great principle of adheſive attraction every where *. To avoid any further diſcuſſion, I ſhall only requeſt Dr. *Macbride* to produce a ſingle inſtance, in which he has either aboliſhed the affinity of coheſion between the conſtituents of a body, without depriving it of its phlogiſton, or reſtored, without the addition of that principle, the calx of a metal to a ſtate of malleability; and all chymiſts allow that decompoſition, and recompoſition, are the ſureſt means of aſcertaining the ſeveral conſtituents of compound bodies.

Since, therefore, ſo many circumſtances concur in ſhewing that all terreſtrial bodies, on being deprived of their phlogiſton, ſuffer a diſſolution, and that none aſſume their primitive form without the reſtitution of that principle, it unqueſtionably follows that their form of aggregation very much depends on its peculiar modifications. Hence it appears that it is not altogether ſo abſurd, as the mechanical philoſophers have ſuppoſed, to ſay that the fitneſs of bodies for the generation of heat in a mechanical way, in ſome meaſure de-

* Dr. *Wilſon's* Medical Reſearches.

pends

depends on the ſtate of their elemental fire: It would even ſeem that in bodies entirely deprived of phlogiſton, little or no heat can be produced in that way. The calces of metals may be mentioned as a proof of this. The conſtant and uniform effect of perfect calcination is the abolition of all coheſion of parts; whence it enſues that no conſiderable force can be applied to calcined bodies without reducing them to extremely minute parts, and they then become friction wheels, and oppoſe the mechanical generation of heat. It is an axiom in phyſics, that the force of vibration is as the elaſticity and force of coheſion; and therefore heat can no more be excited by attrition or percuſſion in friable bodies, than in liquids, and ſuch, in the ſtricteſt ſenſe, are calcined bodies.

Though various other circumſtances tend to ſhow that the fitneſs of bodies for the generation of heat is intimately connected with the ſtate of their phlogiſton, yet it muſt be acknowledged by the patrons of the chymical ſyſtem, that bodies are not inflammable in an exact ratio to the quantity of elementary fire, which they contain, but are more or leſs ſo according to the nature of the ſubſtances, with which it is combined. Thus, there may

may be as much of the principle of inflammability in a bit of gold, as in a bit of cedar, yet the latter is readily inflammable, the former not so at all.

On examining the properties of combustible bodies, we find that phlogiston may exist in an infinity of states of combination, and present itself in a variety of forms in the boundless range of natural phænomena. A bar of iron moderately heated sets fire to wood and other combustibles, but though ignited almost to fusion, it is incapable of inflaming spirit of wine, the purest of all inflammable bodies. The focus even of a concave reflecting mirror cannot inflame it, if proper care be taken to prevent the action of the fire from producing an absolute flame: whence we learn, that the combustion of bodies neither depends upon the quantity of inflammable matter which they contain, nor on the degree of heat applied to them, but on the mode of combination, or the more or less fettered state of their phlogiston, and the means employed to set it in motion. Is it not then unreasonable to maintain that if fire depended on a particular matter, the mechanical generation of it, should be in exact proportion to the quantity of that matter in the

the bodies acting on each other? If bodies are not inflammable in proportion to the quantity of combustible matter, which they contain, why should we expect that their capability of generating heat mechanically, should be in that proportion?

But to evince what I formerly alleged, that the dispute between the mechanical and chymical philosophers, is in fact rather about words than things, I shall now endeavour to shew, that the dense, subtile, and highly elastic fluid, which the great *Newton*, and all who have contemplated nature with the utmost attention, have denominated *ether*, and considered as the one omnipresent, animating principle of all natural things, upon which every property and phenomenon of material-being depends, is one and the same with the phlogistic fluid. A full demonstration of this proposition would, unquestionably, be of very material advantage in the prosecution of natural knowledge; that however we do not absolutely promise, but apprehend that a due attention to facts will prove it to a high degree of probability.

If it be admitted as an axiom in reasoning, that the same effects in the same circumstances, can only be produced by the same cause, we

we have the justest grounds to believe that the so much talked of *ether*, and the principle of inflammability, differ in nothing but in name. In a former part of this treatise, I had occasion to allege that the phænomena of electricity, which the mechanical philosophers have unanimously attributed to the ethereal fluid, are really produced by phlogiston. What I then asserted, I shall now endeavour to evince by arguments deduced from well known facts and experiments.

1°. It is universally taught by metallurgists, that the calcination of metals depends on no other circumstance than a separation of their phlogiston, and that their revivification can only be effected by a restitution of that principle; but it appears from the experiments of *Padre Beccaria* *, and Dr. *Priestley* ‡, that the calces of metals are restored to their metalline state by the electric spark. Have we not then every reason to believe that the electric matter is really the phlogistic fluid?

2*do*. It was formerly observed that air is diminished in proportion to the quantity of phlogiston, with which it is fraught; and it appears,

* Misc. Taur.

‡ Exper. on Air. *Vol.* I.

appears, from Dr. *Priestley*'s experiments, that common air loses a fifth part of its bulk by having the electric spark taken in it. What the physical cause of this contraction in either case may be, remains yet unexplored; but durst I hazard an opinion on the subject, I should attribute it to the attracting and cementing power of phlogiston, by which the particles of air being brought into closer contact occupy, by consequence, less space. This hypothesis is the more plausible, that it strictly coincides with what was formerly said on the principle of cohesive attraction between the component part of mixts. But it is sufficient, for my present purpose, to have shewn, that the phlogistic and the electric fluid produce the same effect.

3*tio*, In the general account, which was given of phlogiston, it was alleged that it is not only exempt from the common laws of gravity, but is even possessed of a power, which we shall never perhaps be able to explain, of diminishing the specific weight of the compounds into which it enters as a constituent. That the electric fluid is possessed of a similar power, appears from all those experiments, in which the reduction of metallic calces is effected by it; for metals are well

 known

known to weigh more in their calcined than in their malleable ſtate. Beſides an ingenious electrician, Abbé *Nollet*, found that a pigeon loſt a hundred and fortieth part of its weight, and one ſort of bird, a fiftieth, by being for ſome time electrified. He even found that ſeveral young perſons loſt ſeveral ounces more of their weight than they were wont to loſe in the ſame ſpace of time, when not electrified *; and with reſpect to air, it is not only contracted in its dimenſions, but diminiſhed in its weight by the electric ſpark; another very material circumſtance, in which the electric matter and the principle of inflammability agree.

4to. It appears from a number of curious experiments inſtituted by *la Fontana*, and made on ſeveral different ſpecies of animals, that the electric ſhock, and phlogiſticated air, deſtroy animal life in the very ſame manner ‖. In both inſtances, animals, if not inſtantaneouſly killed, die univerſally convulſed.

5to. The phlogiſtic and electric fluid coincide in many other particulars beſide thoſe al-

* Lecons de Phyſique Experimentale.

‖ Recerche Fiſiche.

ready

ready mentioned. Both are attended with light, both produce flame, promote the growth of vegetables, expand fluids, precipitate lime from lime-water, and change the blue colour of liquors tinged with vegetable juices, to red.

Laſtly, The electricity and non-electricity of bodies depend upon the quantity and particular modification of their phlogiſton. Various attempts have been made, and theories offered to explain the ſpecific difference between the conductors and non-conductors of electricity; but after attentively reviewing all the experiments, and collating the facts recited by thoſe truly ingenious philoſophers, *Franklin*, *Nollet*, *Beccaria*, and *Prieſtley*, I am induced to aſcribe the conducting power of bodies to their being ſo fully ſaturated, if I may ſo ſpeak, with phlogiſton, as not to be capable of attracting and retaining any more of it. This opinion is the more plauſible, that numberleſs experiments ſhew that bodies tranſmit the electric matter more or leſs perfectly, in proportion to the quantity of phlogiſton which they, at the time, poſſeſs. Thus the atmoſpheric air, when cold, is electric; when hot, or in any ſhape phlogiſticated, it becomes an excellent conductor.

The ſame holds true with reſpect to glaſs; when cold, it is electrical; when red hot, a conductor. All metallic calces are electric, but, by being fuſed with charcoal, they attract its phlogiſton, and are thereby reſtored to a ſtate of malleability, and non-electricity. Theſe few of the many and various inſtances, which tend to ſhow that the electrical ſtate of bodies chiefly depends upon the principle of inflammability, may, it is hoped, ſuffice to evince the truth of that poſition *.

Thus far, I preſume, our endeavours to reconcile two opinions, ſeemingly different, have been ſucceſsful; for if the electric and phlogiſtic matter be what we have reaſon to believe them, one and the ſame principle, it by conſequence follows, that, ſince the *Newtonian* ſyſtem accounts the etherial medium the efficient cauſe of all the phænomena of electricity, whatever properties and powers

* Mr. *Henley* found that the addition of oil to chocolate, reſtored completely its electricity, when loſt †. As the large proportion of phlogiſton in oil is well known, it ſeems allowable to conclude that the effect, produced in the above inſtance, depended on this circumſtance, and by conſequence, that the electric matter is a modification of the phlogiſtic fluid.

† Phil. Tranſ. *vol.* LXVII.

have

have been aſcribed to it are with equal juſtice and propriety to be attributed to phlogiſton. It is further to be obſerved, that as the electrical aura produces flame, expands fluids, and augments their natural evaporation, and in many other particulars reſembles the action of fire, whatever has been advanced in favour of it, or the ethereal fluid, in this reſpect, operates equally in favour of the principle of inflammability.

The ethereal medium has not only been conſidered by the mechanical philoſophers, as the cauſe of electricity, but alſo as the cauſe of light. In order therefore to evince the identity of the ethereal and phlogiſtic fluid, it will be incumbent upon me to ſhow that light either is phlogiſton, under a particular form of action, or at leaſt, that it abſolutely depends upon that principle.

Here opens a boundleſs field for enquiry and ſpeculation; but, as I have neither time nor genius to proſecute the ſubject, in the manner I ſhould wiſh, and which it deſerves, I ſhall only attempt an inveſtigation of the nature of light ſo far as regards the preſent queſtion. To promiſe any thing new on a ſubject, which has exerciſed the ingenuity of the ableſt philoſophers of every age,

might

might juſtly be deemed vain and arrogant; and therefore, that the reader may not be diſappointed, it is proper to inform him, that the ſole aim of the ſubſequent enquiry is to place ſome conteſted points in a clearer light than they have hitherto appeared in; and to ſhow that the *materia ſubtilis* of *Deſcartes*, the ethereal medium of *Newton*, and the phlogiſton of the chymiſts, are materially, if not formally, the ſame.

What is light? How is it formed? and of what ſubſtance? Theſe are queſtions, which have been canvaſſed and diſputed with great eagerneſs ſince the firſt origin of ſcience and philoſophy; and numberleſs are the conjectures which at different periods, have ariſen concerning them, in the ſchools of learning. *Ariſtotle* informs us that *Empedocles*, one of the earlieſt philoſophers of *Greece*, taught that light was an emanation of certain luminous atoms ſubtle enough to pervade the inviſible pores of air, water, and other diaphanous bodies. *Plato* ſeems to have been, in every material circumſtance, of the ſame opinion: and further maintained, that colour is no more than an extremely rare and ſubtile flame, capable of penetrating the denſeſt bodies. The ſame doctrine has been ſtrenuouſly

ouſly ſupported by *Leucippus*, *Epicurus*, and *Lucretius*, and ſeems even to coincide with that of *Democritus*. With reſpect to *Ariſtotle*, he defines light to be the act of a tranſparent body conſidered as ſuch; and adds, that light is not fire, nor is it any thing bodily radiating from the luminous body, and tranſmitted through the tranſparent one, but the mere eſſence of fire, or ſome other luminous ſubſtance at the tranſparent body. This ſeems to have been the real opinion of the founder of the Peripatetick ſchool, though many of his followers have miſtaken it, and foiſted on him another very different, making light and colours qualities of the luminous bodies themſelves, and in every reſpect ſimilar to thoſe ſenſations which they excite in us.

M. *Deſcartes* has refined conſiderably on the *Ariſtotelian* doctrine. He maintains that light, as it exiſts in the luminous body, is nothing but a power or faculty of exciting in us a very clear and vivid ſenſation; and that the inviſible pores of lucid bodies are pervaded by a certain ſubtile and highly elaſtic matter capable of being impelled by theſe bodies, and of producing on the organs of viſion, when properly

perly formed, the perception of light *. Various opinions have been broached on the nature of light, since the days of the vortical philosopher. Some have explained it from a supposed analogy betwixt it and sound ‡. Others have confounded light with air, which they call *spirit*, and think they differ only as metals melted and unmelted, and that light "condensed, storkned and cold, becomes spirit again ‖." But the only opinion which merits to be seriously considered, is that of Sir *Isaac Newton*. That immortal philosopher was the first who discovered the fixed and general laws of light, and proved by the different refrangibility of its rays in passing through a glass prism, that it consists of solid particles of matter. In explaining more particularly the nature of light, he says, "that it is refracted and reflected by an "ethereal medium, by the vibrations of "which, it communicates heat to bodies, "and is put into fits of easy reflection, and "and easy transmission ¶." These are the chief opinions of ancient and modern philo-

* Cartesii Princip. *p.* 3. *sect.* 55. *&c.*

‡ Malbranche. ‖ Hutchinson.

¶ Optics.

sophy

ſophy concerning the nature of light: But as I cannot, without deviating from my intended plan, enter upon a minute diſcuſſion of their reſpective merits, I ſhall content myſelf with obſerving in general, that my notion of light is, "that it conſiſts of certain ſubtile particles "of matter emitted, or reflected, from every "point of the ſurface of a luminous body in "right lines, and in all directions, with an "unparalleled force, and almoſt inconceiva-"ble velocity."

The inveſtigation of the nature and eſſence of the conſtituent particles of light will, I fear, be attended with extreme difficulty. In the peripatetick ſchool, light was conſidered as a ſubſtance neither purely ſpiritual, nor purely corporeal, and was therefore defined a *materia media*; and indeed, when we contemplate, with a philoſophic eye, the aſtoniſhing effects of light, we find ſufficient grounds for accounting it of a nature widely different from lumpiſh, groſs, inactive, matter. That light, however, is material, and its operations merely mechanical, cannot with any ſhadow of probability, be diſputed. The materiality of light not only appears from its being propagated in time, but from its not bending into the ſhadow, which it

ſhould continually do, were it a mere preſſion, or pulſion. The wonderful effects, which it produces on other bodies, ſufficiently demonſtrate that it is a corporeal being. The ſolar rays are not only capable of being collected by a burning glaſs, but, when collected, exhibit marks of a power altogether irreſiſtible. If a diamond, the denſeſt and hardeſt of all terreous bodies, be placed in the focus of a burning mirror, the light immediately enters it, tears its parts aſunder, divides, and diſſolves them. Here we perceive the lens acting upon the light, and the light upon the diamond. Since therefore light both acts, and is acted upon by matter, would it be conſonant with ſound philoſophy to queſtion its materiality? If matter be at all admitted of in the univerſe, we muſt allow its properties to be material, and not ſpiritual. To endue matter with properties, which do not neceſſarily depend on its being ſubſtance, is to poſſeſs it of a ſoul; and to ſubject ſoul to the laws of material mechaniſm, is abſurd and inconſiſtent. So far as we can trace the operations of nature, ſhe proceeds mechanically, and we have reaſon to believe that all her energies depend on the ſame principles. With reſpect to the notion of occult

qualities,

qualities, it muſt have ſprung from the pride and ignorance of man. The more we ſtudy nature, the more we are convinced that all the properties of matter are either chymical or mechanical, I mean, that they are the neceſſary and conſequential effects of the original properties of atoms adhering together in various forms.

It may therefore, I apprehend, without further inveſtigation, be aſſumed as a fact, that light is as certainly a corporeal ſubſtance, as the lens, which converges it, or the diamond it tears aſunder; and that all its properties and effects are the immediate neceſſary conſequences of its material mechaniſm. By dwelling ſo long on the materiality of light, I may perhaps ſeem to have treſpaſſed on the judgment of thoſe, who never called it in queſtion; but as it is ſtill by ſome conſidered as rather partaking of a ſpiritual than bodily nature, I judged it abſolutely neceſſary to remove every doubt on the ſubject, before I ſhould proceed to enquire more particularly into its firſt principles. To have attempted the inveſtigation of a ſubſtance, while it remained doubtful, whether it was body, or ſpirit, would have been truly abſurd.

The unparelleled ſubtlety of light, and the conſequent impoſſibility of ſubjecting it to chymical analyſis, render every enquiry into its eſſence peculiarly arduous and difficult. The only method, therefore, of proſecuting this ſubject with any probability of ſucceſs, is to contemplate light in its effects, to inveſtigate theſe with accuracy and attention, to collate them with ſimilar phænomena, and endeavour to reduce both to the ſame general cauſe. Thus, perhaps, by a ſlow and cautious induction, we may be enabled to form ſome judgment of the matter of which light conſiſts, as well as of the relation which it bears to other ſubſtances in the ſyſtem of nature. It was formerly obſerved that *Plato* defined light, " an extremely rare and ſubtile flame." The truth, however, of that definition has been queſtioned by many celebrated philoſophers, both in ancient and modern times, and continues to this day a ſubject of debate in the ſchools of learning. Whether the ancient ſage, or the impugners of his doctrine have the juſteſt claim to our aſſent, will, I truſt, be determined, with ſome degree of certainty, by contemplating the connection that ſubſiſts between light and fire.

Many and various are the phænomena, which

which point out the moſt intimate and immediate connection between light and fire. We all know that thoſe bodies, wh ch are heated moſt intenſely, are moſt luminous, and that the light of the ſun concentrated by convex glaſſes produces a degree of heat altogether irreſiſtible. Here we perceive that fire produces light, and that light produces the moſt intenſe heat. If, therefore, the ſame cauſes produce the ſame effects, or invert the axiom, if the ſame effects proceed from the ſame cauſes, it muſt in the preſent inſtance be inferred, that light and fire are either one and the ſame ſubſtance, or at leaſt in the immediate chain of cauſe and effect. Both theſe poſitions, however, have been ſtrenuouſly impugned by *Boerhaave*; but, in ſupporting the contrary opinion, he ſeems to have loſt himſelf in a labyrinth of confuſion. He wonders why the rays of the ſun concentrated in the focus of a burning lens ſhould produce ſuch an intenſe heat, while thoſe of the moon, collected by the ſame mirror, have not the leaſt effect upon the moſt ſenſible thermometer *. He thinks he ſolves the problem by ſaying that the rays of the ſun are parallel, while thoſe of the moon are not ſo. That

* Elem. Chym.

how-

however is a fanciful diſtinction. Both the ſolar and lunar rays may be conſidered as parallel, and are in fact phyſically ſo; but ſo great is the difference between the light of the ſun and moon, that hardly any compariſon can be inſtituted between them. M. *Bouguer* found, upon a nice and accurate calculation, that the denſity of the ſolar rays is to the denſity of thoſe of the moon as 3,000,000 to one. But as the light of the ſun when converged, by the beſt mirrors, into a focus, is hardly 300 times denſer than before, and as the degree of heat is always in proportion to the number of rays, what reaſon is there to wonder that the light of the moon, tho' concentrated by a convex lens, does not ſenſibly affect any thermometer conſtructed of ſuch materials, as we are yet acquainted with?

The ſame argument that ſhews the fallacy of Dr. *Boerhaave*'s objections to light and fire being eſſentially connected, will, *a fortiori*, remove the doubts of thoſe, who maintain the ſame opinion, on no other account than becauſe they have obſerved that glow-worms, the *Bolognian* ſtone, and ſuch ſubſtances as are luminous in the dark, have no effect on the moſt ſenſible thermometer. If the

the disparity between the light of the sun and moon be too immense to admit of any comparison, what must it be between the light emitted from that vast luminary, and from a glow-worm? A drop of water compared to the boundless ocean, or a grain of sand to the terrestrial globe, do not afford us an adequate idea of the difference subsisting between them. The connexion of light and fire is further evinced by the well known effects of the latter upon most natural bodies. Innumerable experiments show that there is a certain degree of heat at which bodies become luminous, and that all bodies, which sustain that heat without being converted into vapour, may universally be ignited. There are even some substances, which, though they evaporate at a degree of heat far below that at which they should begin to shine, may, by proper management, be ignited. We have not yet been able to confine water so as to make it red hot; but, that this is not impossible, appears sufficiently from the æolipile vapour, which has been frequently observed to ascend in a blue flame.

Having now pointed out a constant and necessary relation between light and fire, I shall go one step farther, and endeavour to show that

that they are eſſentially, or materially, the ſame. The moſt ſuperficial view of the common phænomena of nature teaches us that light and fire are commutable into each other. The inflammation of all combuſtible ſubſtances generates light; and the light of the ſun concentrated conſtitutes a fire no leſs intenſe than pure. As we do not in the laſt-mentioned inſtance perceive any *pabulum ignis*, we are by conſequence to infer that the ſolar rays do really conſiſt of pure elementary fire. Light, therefore, and fire ſeem to ſtand in the ſame relation to each other, as vapour and water. Concentrated light conſtitutes actual fire; condenſed vapour, water.

It will now, I preſume, appear, that when *Plato* defined light "a rare and ſubtle flame," φλοξ μανη και λεπτη, he came much nearer the truth than later philoſophers have in general imagined. Can we deſire a more convincing proof of the ſolid judgment and penetration of that ancient ſage, than that after the lapſe of ſo many centuries, and the vaſt progreſs made in the ſcience of nature, we are under the neceſſity of rejecting the theories of modern times, in order to revive his long exploded doctrines, as the moſt conſonant with facts and experiments: For if, to the argu-

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ments already brought to evince the identity of light and fire, it be added, that light and heat diffuſe themſelves from a center outward; that they act in ſtraight lines, and are ſubject to the ſame laws of reflection, we can hardly, I think, withhold our aſſent from the *Platonick* doctrine. Before I diſmiſs the ſubject, it will no doubt be expected that I ſhould take notice of two difficulties urged by the impugners of the above opinion. *1ſt.* If light and fire be materially the ſame, whence doth it happen that ſome bodies emit a conſiderable quantity of light without exciting any ſenſible heat? *2dly*, Whence do other bodies on the contrary excite a conſiderable degree of heat, without emitting light?

With reſpect to the firſt of thoſe queſtions, it has been already diſcuſſed at ſufficient length; and as to the ſecond, it will, I apprehend, admit of a ſolution on the fundamental principles of our general theory of light and heat. A late ingenious writer, Mr. *Cole*, has brought ſeveral cogent arguments to prove that the ſenſation of light depends on the number of the particles compounded with the velocity *. But admitting that the

* Obſerv. and Conject. on Light and Comets.

ſame particles ſhould always excite ſenſations both of light and heat, if they were to ſtrike the body with a certain degree of velocity, and in a certain quantity, the reaſon ſtill remains to be enquired, why bodies emit thoſe particles with different degrees of velocity. Though it muſt be acknowledged that we have not ſufficient *data* to determine this point with abſolute certainty, yet various chymical phænomena render it highly probable that the circumſtance, on which, depends the difference, with which bodies emit their igneous and lucid particles, is to be ſought for in the ſtate of their phlogiſton. We find that all bodies, which are readily inflamed, give a ſtrong and copious light, and are ſoon decompounded without producing a very intenſe degree of heat; and that thoſe ſubſtances on the contrary, which are not combuſtible, and are very ſlowly altered by fire, do not begin to emit light till they be intenſely hot, and then do it but ſparingly. Now, as the ſpecific difference between inflammable and non-inflammable ſubſtances depends chiefly, if not ſolely, on the more or leſs fixed ſtate of the phlogiſtic fluid, it is not to be doubted that the difference with reſpect to their exciting of light and heat is to be entirely

tirely attributed to the ſame circumſtance. And in fact, it is notorious that thoſe bodies, which contain moſt of the principle of inflammability in a looſe and ſeparable ſtate, are of all others the eaſieſt inflamed, the ſooneſt conſumed, and emit the greateſt quantity of light. Thoſe on the contrary, in which that principle is ſtrongly fixed by the matters with which it is combined, become intenſely hot before they begin to ſhine. Thus a ball of iron does not appear luminous in the dark, till it has arrived at 635° of *Farhenheit*'s thermometer *, and muſt be heated greatly beyond this before it can be ſtyled red hot. Before iron ſhines brightly even in the dark, it muſt be at grad. 752 ‡; and then, it has no perceptible ſplendour in the twilight, and when heated almoſt to fuſion hardly, in ſtrong day-light, appears ignited. The reaſon of this ſeems to be that, in iron, the phlogiſton is ſo ſtrongly attracted by the other conſtituents, that, till a very high degree of heat be produced, the inteſtine agitation is not ſufficient to extricate that principle; and without a ſeparation of it, no

* Newton. Princip.
‡ Phil. Tranſ. abr. IV.

 light

light is on any occaſion excited. This explanation of the matter is the more probable that iron never reaches the ſhining point of heat, without having its external lamina reduced to a calx; and that will obtain more or leſs according to the degree of ignition, and the time it continues.

Since, therefore, it is apparent that where no decompoſition takes place, no light is excited, and that the more or leſs perfect decompoſition of bodies depends upon the more or leſs ſeparable ſtate of their phlogiſton, it ſeems conſiſtent with facts and experiments, to conclude that the phænomena of light and heat are to be chiefly attributed to the particular modifications of that principle, and to the facility with which it is detached from the other component parts of mixts. Hence light and fire ſtand in the ſame relation to each other in phyſics, as love and luſt do in ethics; for as, according to the poet,

> Luſt, thro' ſome certain ſtrainers well refin'd,
> Is gentle love, and charms all womankind;

So light is fire through ſome certain ſtrainers well refined; that is, both light and fire depend on one and the ſame material cauſe, the phlogiſtic fluid; but this principle ſeems to

exiſt

exiſt in a more pure and ſimple ſtate, or to be leſs incumbered with terrene gravitating particles in the former than in the latter *.

Having now aſſigned the reaſon, why the ſenſations of light and heat are not always conjoined, I ſhall in a few words endeavour to make it appear, that the etherial medium, to which Sir *Iſaac Newton* had recourſe, in order to explain his admirable theory of light and colours, muſt neceſſarily be the ſame fluid as what we now denominate *phlogiſton.* That illuſtrious philoſopher, in his invaluable treatiſe on optics, ſuppoſes that an elaſtic ſubtle medium, which he calls *æther*, is expanded through the vaſt regions of ſpace and pervades all bodies; and that, through it, the rays of light are conveyed from the ſun and

* *N. B.* There is ſtill another circumſtance in which light and phlogiſton ſeem to agree; *viz.* that as the phænomena of light and heat are owing to the phlogiſton being in a certain degree of motion, ſo the lucidity of the particles of light, depends on their velocity; and when this is diminiſhed beyond a certain degree, they are no longer luminous. It is thus only that we can rationally account for the ſudden annihilation of the immenſe quantity of light emitted from the ſun during the day, and for the total extinction of heat on the principle of inflammability becoming fixed in bodies.

other

other lucid bodies to the various parts of the mundane ſyſtem.

In ſupport however of this opinion, he has not advanced a ſingle proof, and ſeems to have given it with no other view, than to ſuggeſt matter for further enquiry. Some late writers nevertheleſs, for reaſons only known to themſelves, have aſſumed the exiſtence of ſuch a medium as a matter of fact, and attempted to account for all the phænomena of light by its different modifications. Had thoſe gentlemen reflected that it is a maxim in the ſtudy of nature, not to multiply agents raſhly and without neceſſity, they moſt aſſuredly would have at leaſt demonſtrated the phyſical neceſſity of ſuch a contrivance, before they aſſumed, as a fact, the exiſtence of an agent, in favour of which, they had not a ſingle argument to offer.

If, according to the *Newtonian* ſyſtem, light be really compoſed of ſolid particles of matter, and projected from the luminous body with an inconceivable velocity, what reaſon have we to ſuppoſe the force with which it is emitted inſufficient to convey it to its place of deſtination? Why call in the aid of a conducting medium, of which we not only have no evidence, but of which we

cannot

cannot even form a clear and diſtinct idea? But did we grant the exiſtence of ſuch a medium, ſhould we not be under the neceſſity of having recourſe to a third agent in order to explain its laws, mechaniſm, and mode of operation? In ſhort, why ſuppoſe light ſo powerful in ſome inſtances, and impotent in others? "Do not (ſays Newton) bodies and light act "mutually upon one another, that is to ſay, "bodies upon light in emitting, reflecting, "refracting, and inflecting it, and light up-"on bodies for heating them and putting "their parts into a vibrating motion, where-"in heat conſiſts? And are not groſs bodies "and light convertible into one another, and "may not bodies receive much of their ac-"tivity from the particles of light, which "enter into the compoſition *?" Do not theſe mark the power and activity of light, and ſhew that it pervades all bodies whether diaphanous, or opaque, and with this difference only, that it is tranſmitted through the former, but ſtops and becomes fixed in the latter? It is ſurely then more conformable to the ſimplicity of nature to aſcribe all the well-known properties and energies of light to

* Optics.

the powers inherent in, and inſeparable from its nature and conſtitution, than to attribute them to a medium of the exiſtence of which, we perceive as little neceſſity, as we find proofs.

Though I have already pointed out a variety of circumſtances, tending to ſhew that the chief debates that ſubſiſt between the chymical and mechanical philoſophers turn more on words than things, I cannot diſmiſs the ſubject without endeavouring to evince, what I have often hinted, that the matter of light either is, or contains, phlogiſton; and that to this principle is owing both the ſenſation of light, and heat. It was formerly obſerved, that as we perceive nothing during actual inflammation, but a ſtream of light and heat, we muſt, if we truſt to our ſenſes, neceſſarily conclude that light and heat are excited by phlogiſton, or in other words, that the ſame matter, which in a ſeparate ſtate, produces theſe phænomena, is, when modified in bodies, the principle of their inflammability *. If we ſurvey the various operations

* Our phlogiſton, ſays M. *Senebier*, is to ordinary fire what light is to phlogiſton. Ordinary fire goes out without aliment; its intenſity varies; it is produced by the

operations of nature with that attention and accuracy, which are neceſſary in the proſecution of phyſical inveſtigations, I venture to ſay, that we ſhall not meet with a ſingle inſtance, from which it will appear that heat or light can be excited without the manifeſt concurrence of this elementary principle of fire. Every part of nature affords facts in ſupport of this opinion. Contemplate the great luminary, which enlightens the univerſe, and without whoſe genial warmth both animal and vegetable life muſt ceaſe, and all nature become one lifeleſs, torpid, diſmal ruin, and it will be found that every ray is fraught with the phlogiſtic fluid.

Various ſubſtances, merely upon being expoſed to the ſolar beams, attract a quantity of that principle, which becomes fixed and produces the ſame effects, as if an inflammable matter had been united to them. Of this, among numberleſs others, the *Phoſphorus Bononienſis* is a no leſs curious than ſtriking inſtance. It is a ſpar found in the neighbourhood of *Bologna*, containing a large

the expanſion or developement of phlogiſton, occaſioned by the diſſolution of the body which contained it. *Vide* Memoir ſur le Phlogiſtique.

quantity of gypſeous matter. When properly calcined, it, on being expoſed to the light of the ſun, acquires the property of ſhining in the dark; nay, it even yields in the dark, after being expoſed to the coloured rays of the ſun, the ſame coloured light to which it had been expoſed. This was firſt obſerved by Father *Beccaria* of *Turin*, and confirmed by Profeſſor *Allamand* in *Holland*. It is, however, to be remarked that hot ſun-ſhine deprives this *ſpongia ſolis* of its quality of emitting light, till it be calcined a-new, and conſequently for the experiment to ſucceed, it is neceſſary the expoſition ſhould be made in a ſhady place.

The nature of this ſingular phenomenon can only, I think, be explained in the following manner. The ſtone being robbed of its phlogiſton by calcination, attracts, when expoſed to the ſolar rays in a ſhady place, a ſmall portion of that principle, which being inſufficient to reſtore the affinity of coheſion among the parts, remains in a very looſe and ſeparable ſtate; whence it happens, that the ſtone being brought into a medium that has a ſuperior attraction for the principle of inflammability, this flies off, and in its flight becomes viſible; but, by being expoſed for a

conſiderable

considerable time to the direct rays of the sun, it imbibes and fixes such a quantity of phlogiston, as in some degree to regenerate those original properties, of which it had been deprived by the loss of that fluid in calcination.

Dr. *Black* has endeavoured to account for this singular property of the *Bononian* phosphorus, by saying that the phlogiston of the charcoal, used in the calcination, is united to the acid of the gypseous matter, and becomes brimstone, but still adheres to the calcarious earth, and this, by its action upon the acid, diminishes the cohesion between it and the principle of inflammability, in such a manner, that the particles of light are sufficient to dispose it to be inflamed. But granting that to be the case, why should not a degree of heat equal to that derived from the heat of the sun produce the same effect, as it does? Besides, how, on Dr. *Black*'s principles, is the abolition of the phosphorical quality of the *Bologna* stone by the direct rays of the sun to be accounted for? In short, if we suppose a transmutation of the inflammable principle of the phosphorus into light, how shall we be able to account for its emitting the same particles of light, which it had imbibed;

bibed; ſo that if red rays only are thrown upon it, it appears red; and if it be expoſed to blue rays only, it appears blue? And till theſe queſtions be ſolved, this opinion, however ingenious, muſt be inadmiſſible. That this phenomenon is not owing to the heat of the air, as he ſuppoſes, but to the phlogiſtic fluid in the ſolar rays, is further confirmed by ſeveral ingenious electrical experiments inſtituted by Mr. *Lane*, and related by Dr. *Prieſtley* *. The above gentleman obſerved that by making the electric ſpark paſs over any kind of gypſeous earth, and calcarious ſubſtances, whether animal or foſſil, the part over which the aura had paſſed was luminous, and retained that appearance for ſome time. Mr. *Lane* performed the above experiment on the *Bononian* ſtone, and found that the electric ſpark produced the ſame effect as the light of the ſun.

It may be brought as a proof of the preſence of the inflammable principle in the rays of the ſun, that combuſtible bodies of every kind are conſumed with much greater rapidity in the night, than in the day; in the

* Hiſtory of Electricity.

ſhade

ſhade than in the ſun. The reaſon, though perhaps never attended to, is obvious. It is the nature of all inflammable ſubſtances not to part with the phlogiſtic fluid, unleſs the ſurrounding medium attract it more powerfully than they do. Hence the air in the direct rays of the ſun, being in ſome meaſure ſaturated with that principle, draws it but ſlowly from the bodies with which it is combined: For inflammation on all occaſions will be more or leſs rapid, according to the quantity of phlogiſton, with which the circumambient air is impregnated.

Phytology likewiſe affords ſeveral facts which plainly tend to corroborate the foregoing opinion. Experience has taught us that light is as much and perhaps more neceſſary to perfect vegetation, than the atmoſpheric air. There are a number of experiments publiſhed in the memoirs of the French academy, which were inſtituted with no other view than to aſcertain the cauſe of the green colour ſo univerſal in vegetables. Various theories had been offered to explain the fact. Some had ſuppoſed it owing to the univerſal aerial acid; ſome to an alkali; and others to a ſmall portion of ferruginous matter diſcoverable in vegetable bodies on a chymical analyſis; but

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the following circumſtances will, I apprehend, leave no room to doubt that the green colour of plants depends entirely on the ſolar light.

It appears from inconteſtible facts, that the root of the moſt variegated flower, though excluded from the external air under a glaſs veſſel, will, provided it be daily expoſed to the light of the ſun, arrive at its utmoſt perfection with reſpect to fragrance and colour; but if the proceſs be reverſed, and the air admitted without the light, the flower may perhaps grow to its natural ſize, but we in vain ſhall look for that beautiful variety of vivid colours, and for that exquiſite perfume, which nature beſtows on every individual of the ſpecies, when permitted to imbibe from the ſolar beams the phlogiſtic fluid. The ſame fact is further evinced by a variety of experiments performed by ſeveral French academicians, in which the light was admitted to one part of a plant, and excluded from the others. The invariable effect of this was, that the part expoſed to the ſun became of a lively green, and that, which was ſhaded, continued of a diſagreeable pale colour: nay, ſo powerful are the effects of the ſun's light on vegetables, that when deprived of it, their

their taſte and other native properties undergo ſuch a change, that ſome in their nature poiſonous, become a ſafe and wholeſome food. On this is founded the ſecret of *blanching*.

All theſe circumſtances evidently ſhew that there is ſomething in light abſolutely neceſſary to vegetable life. It is no doubt on this account that all plants ſhew a remarkable ſenſibility to the light. They expand their leaves, and open their flowers to the ſun, and the moment he diſappears, they cloſe them. This curious phenomenon is not to be explained on mechanical principles. Many accurate experiments ſhew that it is not the heat, but the light of the ſun, that cauſes them to turn to him. A plant in a room, where there is a fire, turns its flowers to the light, which comes from the colder ſide. Beſides this influence of light on the growth of plants, it ſeems to contribute to their nouriſhment. It is well known that various vegetables grow in water alone, provided that they be expoſed to the light: nay, it is highly probable that trees and plants derive ſo great a part of their nouriſhment from light and water, that if they were not deſtroyed by man and other animals, but ſuffered to

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corrupt on the ſurface of the earth, they would return with increaſe into its boſom, and feed rather than impoveriſh it. But tho' we can readily conceive that plants growing in water obtain their terrene parts from it, which not only diſſolves earth, but is convertible into it by fire and trituration, yet it is not ſo eaſy to explain whence they derive the inflammable matter, which they all without exception contain.

It has been already ſhewn, that the green colour of vegetables is owing to the ſolar light; and the ſubſequent facts will, I apprehend, leave little room to doubt that their inflammable matter comes from the ſame ſource. If the juice of a ſucculent plant, ſuch as lettuce, be expreſſed in a mortar, it will at firſt appear uniformly green; but, if permitted to ſtand, the green part ſeparates from the watery, and forms a ſediment, which, when collected and ſubjected to further experiments, is found to be of an oleaginous nature, for it will not diſſolve in water, but readily in oil, or ſpirit of wine, to which it gives a green colour. Here then we find that the light of the ſun produces the green colour of plants, and that the ſubſtance in which it reſides ſeems to be an oil;

and may we not therefore with great justice conclude that the light furnishes the oleaginous matter, and is the chief if not sole cause of its introduction into vegetables.

We shall have little difficulty in subscribing to the above opinion, if we reflect that light is capable of being attracted by metallic calces, and of restoring to them the principle of inflammability. We are likewise by analogy furnished with several arguments in support of the same position. It was evinced in a former part of this treatise, that the phlogistic fluid is really the colouring matter in the bile and blood, and that by depriving them of it, their taste, colour, and other sensible qualities are utterly destroyed: and since, by excluding the solar rays from growing plants, they never acquire their natural colour, taste, or smell, are we not warranted to infer that, as those qualities in other substances depend on phlogiston, it is this principle which light imparts to vegetables *. This deduction will

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* Colour, considered in the coloured body, is, according to *Newton*, no more than a certain disposition of its surface, which fits it to reflect such and such rays of light; but the chymist, who pervades the intimate texture of bodies, discovers something more. He finds

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not appear rash or groundless to those, who have observed that atmospherical air charged with phlogiston, powerfully promotes vegetation, and would perhaps, if properly and plentifully supplied, compensate the absence of the solar light. Various experiments also prove that the electrical aura is no less friendly to the growth of plants, than phlogisticated air: a fact, which evidently tends to shew that the inflammable matter of vegetables is derived from the sun.

Vegetation may justly be reckoned the great provision in nature for preserving and restoring the salubrity of the atmosphere. This curious fact was accidentally discovered by Dr. *Priestley*; but he doth not attempt to explain it. A late ingenious writer seems

that the blue colour of argil depends on a metallic matter, of which he, at pleasure, can deprive it; and by a farther analysis, that this matter derives its colour from the phlogiston which it contains. He knows, in short, that he can add colour to some bodies, change it in others, and separate it from all. Nor is it contrary to the *Newtonian* doctrine, as many have erroneously supposed, to maintain that colour depends on a particular matter; it is only rising a step higher in the infinite ladder, and showing that the disposition of bodies to reflect particular rays of light, depends on a physical being, and no occult quality of matter.

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inclined to aſcribe a conſiderable ſhare of this grand and important effect to evaporation, whoſe operation is ſo extenſive on the ſurface of our globe, that it not only obtains in hot and temperate climates, and on the fluid ocean, but alſo in thoſe frozen regions of the earth, where few vegetables appear, and billows never roll. But the melioration of tainted air by vegetation admits, in my opinion, of a more ſatisfactory explanation on the following principles *. It is well known that plants thrive, in a moſt ſurpriſing manner, not only in air tainted by the putrefaction of animal and vegetable ſubſtances, but likewiſe in air vitiated by reſpiration, and phlogiſticated by flame. Now, as on moſt of thoſe occaſions the noxious air is reſtored to a reſpirable ſtate, by being robbed of the phlogiſton, with which it was loaded, it is obvious that this effect is produced by the plants: and hence, that ethereal fluid may with ſufficient accuracy be ſtyled the chief aliment, if not the very ſoul and life of vegetables, and is unqueſtionably the identical matter, which they imbibe from the light of the ſun. This is perfectly conſonant with Dr.

* Treatiſe on permanent elaſtic Fluids.

Franklin's opinion, who, as was formerly obſerved, thinks " that the fluid *fire*, as well as " the fluid *air*, is attracted by plants in their " growth, and becomes conſolidated with " the other materials of which they are " formed, and makes a great part of their " ſubſtance *." Sir *Iſaac Newton* too ſeems to have apprehended the ſame thing. " Are not (ſays he) groſs bodies and light " convertible into one another? The change " of bodies into light and light into bodies is " very conformable to the courſe of nature, " which ſeems delighted with tranſmuta- " tions ‡."

The effects of fixed air with reſpect to vegetation, and the preſervation of plants ſeem not yet ſufficiently aſcertained. The experiments inſtituted by Dr. *Prieſtley* and Dr. *Percival*, with a view to determine that point, are too contradictory in their reſults to admit of certain concluſions being deduced from them. But, *a priori*, I am inclined to believe that fixed air can have very little effect in promoting vegetation. If the phlogiſtic fluid be really the chief pabulum of vegetable life; a thing, which I preſume has

* Letters. ‡ Optics.

been

been ſufficiently evinced, it is manifeſt that, as fixed air contains only a very ſmall portion of that principle, it will afford little or no nutriment to growing plants. It is even reaſonable to ſuppoſe that this ſpecies of air, from the nature of the ſubſtances of which it is, on many occaſions, produced, may be rather detrimental than friendly to vegetable life.

Such are the obſervations and conjectures, which occurred to me on the nature and energies of light; and I am perſuaded that whoever ſhall weigh, with impartiality and attention, the foregoing facts and phænomena, will acknowledge the preſence of phlogiſton in the ſolar beams; and ſince it has never been ſhewn that they are fraught with any other matter, it may not only be inferred that light is the principle of inflammability under a particular form of action, but likewiſe that the heat of the ſun is entirely owing to that fluid: and this leads me to the laſt and moſt material point, about which the mechanical and chymical philoſophers have never been able to agree. The reſpective opinions of theſe two ſects on the ſubject of heat have been already explained at ſome length. The former maintain that heat conſiſts in motion

alone; the latter, that it depends on an elementary principle of fire. No two doctrines can to appearance be more dissonant from one another; and yet I do not despair of shewing that they are at bottom very nearly the same. How paradoxical soever this position may at first sight appear, a little attention, and nearer view of the contested point will, I trust, render it extremely probable.

Sir *Isaac Newton*, the illustrious leader of the mechanic system, was not only obliged to call in the aid of an ætherial medium to explain his doctrines of light and gravitation, but had likewise recourse to the same subtile and elastic fluid, to account for the phænomena of fire and heat: And accordingly the reason he assigns why sulphureous bodies take fire more readily, and burn more vehemently, than other bodies, is, that "they are acted "on, by the ætherial medium, by which "light is refracted and reflected, and by the "vibrations of which light communicates "*heat* to bodies. And do not the vibrations "of this medium in hot bodies contribute to "the intenseness and duration of their heat? "And do not hot bodies communicate their "heat to contiguous cold ones, by the vibrations of this medium, propagated from

"them

"them into the cold ones? And is not this "medium exceedingly more rare and subtle "than air, and exceedingly more elastic and "active? and does it not readily pervade all "bodies *?"

It is therefore unquestionably true, that Sir *Isaac Newton* accounted the ætherial medium necessary to the production, duration, and propagation of heat; and since it appears from numberless facts and phænomena, that his æther is the same principle as the phlogiston of the chymists, it by consequence follows, that the tenets of the chymical and mechanical philosophers, on the subject of heat, are by no means so widely different, as they have been generally imagined. To be warranted to draw this conclusion, has been the chief aim of all that has been advanced respecting those two opinions: How far I have succeeded in my attempt to reconcile them, the learned and philosophic will determine. In the mean time, I cannot help regretting with every lover of science, that men of real genius should waste so much of that time and erudition which might be successfully employed in the prosecution of useful know-

* Optics, *quær.* 18.

ledge,

ledge, in diſputes, which at laſt are found to have turned merely upon words. This, it is hoped, will now appear to have been the caſe in the preſent inſtance.

It might reaſonably have been expected that in a queſtion, which has been canvaſſed and diſputed with great eagerneſs ſince the firſt dawn of ſcience and philoſophy, the diſputants ſhould long ere now have been able to paſs from words to the true and real ſubject of the controverſy; but unfortunately, after ſo long a ſeries of years, we ſtill find them on the very ſpot on which they firſt ſtood. Had they ever underſtood each other, the queſtion, in all probability, would not have remained till now undecided. The mechanical philoſophers would have perceived that the difference between their æther and the phlogiſton of the chymiſts is merely nominal; and the chymiſts would have acknowledged that motion is abſolutely neceſſary to the production of fire. For it is only when the phlogiſtic fluid is actually in motion that the phænomena of light and heat take place; and whether this motion, the nature of which cannot be aſcertained by experiment, be termed vibratory, or not, is a circumſtance of no importance to the queſtion.

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It is likewiſe equally immaterial, whether the fluid in motion be denominated æther, or phlogiſton, provided we underſtand each other.

The following curſory obſervations on the various ways in which heat may be excited, will, I truſt, throw ſome farther light on the doctrine, I wiſh to eſtabliſh. It is difficult to give a conciſe and accurate definition of heat, owing to the ambiguity of the word. By ſome it is defined one of the ſecondary qualities of bodies produced by fire and oppoſed to cold. According to *S. Graveſande* *, and others, heat in the hot body is an agitation of the parts of the body made by means of the fire contained in it, in conſequence of which, a motion is produced in our bodies, which excites the idea of heat in our mind. Heat, then, as well as light, gravitation, and attraction, is promiſcuouſly uſed both for cauſe and effect; and for this reaſon, I have generally meant by heat not merely the perception, but that particular ſtate of fire in bodies, in conſequence of which, certain ſubtle particles are emitted, which, if they happen to fall on the ſentient extremities of

* Inſtit. Phil. Natur.

our nerves, excite in our minds the idea of heat; if on brute inanimate matter, they dilate, expand, dissolve, or divide it.

The first and most remarkable source of heat upon the surface of the terraqueous globe is the sun. Did he refuse to shed his benign influence around, the beauteous face of nature would be at once destroyed, all order would be subverted, animals and vegetables must instantly perish, the rolling ocean would become a mass of ice, and the superincumbent atmosphere, a dense and solid body. That these effects would be the immediate consequence of the absence of the solar heat is not to be questioned. But on what doth this heat depend? Is it on the rapid motion of the rays of light alone, or on the action of the matter of which they are composed? These questions, on the principles we have laid down, are easily resolved.

Heat, we have seen, in no one instance depends on motion alone. To account for its production, the mechanical philosophers have recurred to an ætherial medium, the chymical to a phlogistic fluid; and as both these fluids coincide in every effect, and phenomenon, ascribed to them by their respective advocates, we are induced to believe that

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the difference between them is merely verbal. Farther, ſince the degree of heat communicated to bodies is in proportion to the quantity of light falling upon them, and that every ray projected from the orbit of the ſun is fraught with phlogiſton, and with no other matter whatever; are there not ſufficient grounds to infer that the ſolar heat doth really and ſolely depend upon the action of that principle? This opinion will be farther confirmed and illuſtrated by exploring the different modes, in which artificial heat may be generated. On a ſuperficial view of nature, there would ſeem a thouſand various ways of exciting heat; but on a nearer ſcrutiny, we ſhall find that they are not only all reducible to a few general heads, but likewiſe that they all, without exception, depend on one individual circumſtance, *viz.* the evolution or extrication of the principle of inflammability from the compounds into which it enters as a conſtituent. There are properly two ways only, in which this can be effected. The firſt is by the introduction of fire *ab extra*; the ſecond by the inteſtine motion of the parts of mixts ſuperinduced by the concurrence of internal as well as external cauſes.

1*mo.* When we apply an ignited bar of iron to any dry combuſtible body, ſuch as wood, we immediately ſet it a-flame. This, which is given as an inſtance of the firſt mode of producing fire, may be explained in the following manner. The moment the dry wood comes within the ſphere of activity of the igneous particles emitted from the iron, it is pervaded by them with reſiſtleſs impetuoſity. The phlogiſton preſent in it is thrown into a violent commotion, the attraction of coheſion is by conſequence diminiſhed, and the principle of inflammability being at laſt diſentangled, flies off in a pure and active ſtate, under the ſenſible phænomena of light and heat. Hence we conceive why certain bodies, ſuch as ſulphur, ſpirits, oils, pitch, *&c.* were in the *Ariſtotelian* ſchool ſaid to be virtually, or potentially, hot. Theſe bodies indeed contain a large proportion of elementary fire, but contain it in ſo fixed and fettered a manner, that, unleſs by ſome external cauſe it be excited into action, and extricated from the materials with which it is combined, no actual heat is generated.

2*do.* Heat, however, may in ſome bodies be excited without the immediate application

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of any considerable quantity of fire from without, *viz.* by fermentation and mixture. By fermentation is meant an intestine motion, which, arising spontaneously among the insensible parts of a compound body, produces a new disposition, and a different combination of those parts. By effervescence, we understand an intestine motion excited among the parts of two bodies of different natures, when they reciprocally act on each other. Of all natural bodies, vegetables are alone susceptible of the three different stages of fermentation; the vinous, acetous, and putrefactive. Animal substances are capable only of the latter; and fossils of none. The chief phænomena in fermenting liquors are a brisk intestine motion of their parts in all directions, accompanied with a muddiness, a hissing noise, the exhalation of fixed or mephitick air, and a gentle heat. An investigation of the principles, on which this last-mentioned circumstance ought to be explained, is the sole aim of the present enquiry.

It has been already observed, that there enters into the composition of all vegetables a large proportion of inflammable matter; but, though in a constant *conatus* to motion, it

it is fettered in ſuch a manner by the other components of the maſs, that the concurrence of ſeveral circumſtances is abſolutely requiſite to ſuperinduce that particular ſtate, in which, without being actually inflamed, it excites heat. Fermentation is that particular ſtate in plants, and ſeveral things are neceſſary to its taking place. Theſe are, in all ſaccharine ſubſtances, plentiful dilution, and in the farinacious, malting, or a certain degree of vegetation; and in both, a temperate heat, either natural, or artificial. By dilution, the attraction of cohesion among the parts is diminiſhed; for as water and earth powerfully attract each other, the affinity of the latter to the phlogiſton will be proportionably weakened; and as the external heat by expanding and ſeparating the air with which vegetables abound, ſtrongly promotes the internal agitation and decompoſition of the mixt, the inflammable matter is gradually extricated and reduced to an active ſtate, and on this, as on all ſimilar occaſions, excites a ſenſible degree of heat. This account of the matter is ſupported by numberleſs facts and phænomena. The heat generated during the fermentation is always proportionate to the rapidity with which the proceſs

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is carried on. The heat generated in a maſs of green hay ſometimes ariſes to actual inflammation: But in a diſtiller's vat, when the proceſs is properly conducted, the heat of the fermenting liquors is very nearly that of the human body *.

The decompoſition of vegetable ſubſtances by fermentation, is attended with ſo great an exhalation of their volatile parts, that at laſt ſcarce any thing remains but a light ſpungy maſs of earth. During the putrefactive ſtage vapours ariſe, which are extremely offenſive. Theſe have been aſcribed ſolely to the volatile alkali produced by the ultimate reſolution of animal and vegetable bodies; but beſides it, there ſeems to be another matter, to which the ſmell ought partly to be attributed. I mean the principle of inflammability combined with ſome other matter. We are led to this opinion from a variety of circumſtances. If the vapour ariſing from putrefying animal or vegetable ſubſtances be brought into contact with the calces of ſome metals, it tinges them in the ſame manner as if they had been expoſed to the fumes of burning ſulphur, or any other obvious proceſs

* Franklin's Letters.

of

of phlogiſtication; and it would, I doubt not, if properly applied and long continued, reſtore the calces of ſuch metals, as are eaſily reduced, to their ductile ſtate. Another phenomenon, which indicates the preſence of the phlogiſtic fluid in the vapours emitted from putrefying bodies, is their readineſs to be ſet on fire. The ſubtle exhalations, which ariſe from the cotton wood in a putrid ſtate, not only appear luminous in the dark, but, when collected, are found to be no leſs inflammable than the fumes of diſſolving metals *. There is upon the whole a ſtriking analogy between the reſolution of animal and vegetable ſubſtances by fermentation, and the ſpontaneous decompoſition of native pyrites, or a mixture of ſulphur, iron, and water. In both caſes, the decompoſition is accompanied by a ſtrong inteſtine motion of the parts, by the emiſſion of inflammable vapours, by actual heat, and new combinations of the original conſtituent principles.

From the curſory view which has been taken of fermentation, and the circumſtances attending it, we can hardly doubt that the heat produced on the occaſion is the con-

* Mr. Cavendiſh's Phil. Tranſ. *paſſim.*

ſequential

ſequential effect of the actual extrication of phlogiſton from the materials with which it is combined. In the firſt place, we have ſeen that no body whatever is ſuſceptible of fermentation, that doth not contain a certain quantity of the principle of inflammability. In the vinous ſtage, when properly conducted, there is only a ſmall portion of phlogiſton diſſipated. The degree of heat applied is too inconſiderable to ſet it entirely at liberty, and the cloud of fixed air that hangs over the fermenting liquor prevents its flight into the ſurrounding atmoſphere; hence it is forced to become a conſtituent of the new mixt. And were proper means taken to obviate the ſtagnation of the fixed air over the vat, the contained fluid would become abſolutely vapid, and, on diſtillation, afford little or no ardent ſpirit.

2dly, In the acetous ſtage, the degree of heat generated is more conſiderable, than in the vinous. Two remarkable circumſtances render it highly probable, that the ſuperior degree of heat attending this proceſs is owing to a more rapid evolution of the phlogiſton, and its more compleat ſeparation. The one is, that the quantity of fixed air produced is not ſufficient to obſtruct the diſſipation of

that principle; the other, that all liquors, which have gone through this second stage of fermentation, yield, when distilled, no inflammable inebriating spirit.

During the last stage of fermentation, there obtains hardly any sensible degree of heat. This fact M. *Beaumé* has taken much pains to ascertain without investigating its cause *. The most probable conjecture seems to be, that the greatest part of the inflammable principle is, in vegetables, dissipated during the two preceding stages; and in animal substances, that the decomposition goes on so slowly, and the phlogiston flies off so clogged with the volatile alkali, that it properly never is in that free and active state in which it excites heat.

The last source of heat we shall mention is chymical mixture. In the whole history of nature there is not a more remarkable phenomenon, than the production of high degrees of heat, and even a bright and lasting flame, from the mixing of two cold liquors. We have a striking instance of this in the combination of concentrated nitrous acid with distilled oils. The only principles on which this fact admits of a probable solution, are those of corpuscular attraction.

* Mem. de l'Acad. des Sciences.

Before

Before the days of Sir *Iſaac Newton* all the phænomena of nature were explained on mechanical laws alone. The action of all chymical menſtrua was attributed ſolely to the different ſhapes and ſizes of their component particles, and the different motions, by which they were agitated. Some were ſuppoſed cuniform, ſome pyramidal, and others obtuſe and ſpherical: But ſince the *Newtonian* ſyſtem was known, the idea of ſuch particles has been reprobated by every rational chymiſt. That great philoſopher having diſcovered that by gravitation the motion of the ſolar ſyſtem is effected, eſtabliſhed, and conſerved, was naturally led to believe all matter endued with a ſimilar property; and therefore attributed the union of aggregation, the action of capillary tubes, the adheſion of ſmooth ſurfaces, magnetiſm, electricity, chymical ſolution, and the efferveſcence of mixts, to a ſtrong attractive power mutually exerted between the primigenial corpuſcles of matter. The heat generated by chymical menſtrua, he attributes to the friction of the integrants in conſequence of the impetuoſity with which they ruſh together.

This theory was received by the unprejudiced with pleaſure, as affording a ſatisfactory

ſolution of many difficulties, with which former hypotheſes were clogged. It is not however unexceptionable in every particular. It is neither ſtrictly true with reſpect to the cauſe of effervefcence, nor the production of heat. It is now well known that effervefcence, and ebullition, are occaſioned by the air, or other elaſtic gas, which eſcape during the ſolution; and with reſpect to the heat produced on thoſe occaſions, it aſſuredly depends on ſome other cauſe than the mere attrition of the colliding particles. Were heat owing ſolely to that circumſtance, none ſhould be excited, when, by the union of two fluid bodies a denſe compound is produced: of which, however, we have many notable inſtances in chymiſtry. The *Newtonian* doctrine, on this head, is the more exceptionable, that all the chymical mixtures accompanied with heat, accord in one very ſtriking and ſeemingly eſſential circumſtance; I mean in giving indubitable proofs of the preſence of the inflammable principle. I have taken ſome pains to collect all the inſtances in which heat is produced by mixture, and do not find one, in which that phenomenon occurs without the above-mentioned circumſtance: Nay, I have generally found that thoſe ſubſtances, which contain

contain the greateſt proportion of phlogiſton, produce, in ſimilar circumſtances, the moſt heat in efferveſcing with acids. By reaſoning on theſe facts, we are enabled to account in a ſatisfactory manner for the accenſion of diſtilled oils by means of the concentrated nitrous acid.

It is notorious that of all chymical bodies, the nitrous acid manifeſts the ſtrongeſt attraction for the phlogiſtic fluid. It is alſo known, that both the eſſential and empyreumatic oils contain a very large proportion of that principle, and contain it in a more or leſs ſeparable ſtate according to their degree of purity. We have, therefore, every reaſon to believe that the heat attending the combination of thoſe bodies is the immediate effect of the rapid extrication of phlogiſton from the oil, in conſequence of the ſuperior attraction which the nitrous acid hath for it. Did the heat excited originate ſolely from the collustation and friction of the integrants, ſhould it not take place in the ſame degree, when thoſe oils, though leſs pure and dephlegmated are of the ſame conſiſtency? This however is by no means the caſe; for unleſs their watery and incombuſtible parts be carried off by diſtillation, and the principle of inflammability left

left in a looſe concentrated ſtate, the ſtrongeſt nitrous acid doth not produce actual flame. Upon the whole, we are led to conclude that as often as heat is the effect of mixture, there is uniformly a manifeſt extrication of phlogiſton; and that the degree of heat produced is generally in the compound ratio of the quantity of that principle, in a ſeparable ſtate, and the power of attraction, which the body ſeparating it has for it.

CHAP.

CHAP. V.

The Heat generated by the Evolution of Phlogiſton from the Blood, is ſufficient to account for the Temperature of living Animals.

HAVING, in the preceding chapter, not only ſhown from various natural phænomena, and chymical facts, that heat is never generated where phlogiſton is not preſent; but likewiſe, that the extrication of that principle from the ſubſtances with which it is combined, is generally attended with a degree of heat proportionate to the quantity extricated, it may reaſonably, I preſume, be inferred, that the developement of the phlogiſtic fluid, which takes place in the living ſyſtem, muſt have ſome ſhare in generating the heat of animals: and if it be admitted that it produces this effect in any degree, we may venture a ſtep farther, and ſuppoſe it the ſole cauſe of animal heat.

This

This propoſition cannot be demonſtrated, but will be found conformable to Sir *Iſaac Newton*'s method of induction.

Convinced that nature is ſimple, and delights not in ſuperfluous cauſes of things, Sir *Iſaac* laid it down as a fundamental rule in philoſophizing, that of natural effects of a ſimilar kind, the ſame cauſes are to be aſſigned; as of light and heat in the fire, and in the ſun: And although the arguing from experiments and obſervations by induction, be no demonſtration of general concluſions, yet it is the beſt way of arguing, which the nature of things admits, and is valid according to its generality; and when no diverſity is found in the phænomena, the concluſion muſt ſeem, even to the moſt ſceptical mind, in a high degree probable. Now to require demonſtration in the preſent queſtion, which relates only to facts, would be abſurd; for arguments, which are either demonſtrative, or dialectical, have each their peculiar object. The demonſtrative make us acquainted with the relations of ideas, and are independant on exiſtence: The dialectical are employed wholly on facts, and beget belief or opinion. Experience, therefore, and analogy, are the ſource of the latter, and in the inveſti-

investigation of nature are our only guides: and hence, the truth of theories, on natural subjects, arises from the uniformity of experience and analogy.

As we advance in the study of nature, we daily find more reason to be convinced of her constancy in all her operations; that like causes in like circumstances always produce like effects; and inversely, like effects always flow from like causes. The inconstancy, which appears at first in some of nature's energies, a more improved experience teacheth us to explain. The more, then, we become acquainted with elementary principles, the more we are convinced by a general experience of the uniformity of their operations. Since therefore it has been evinced from innumerable concurring facts, that the presence of the phlogistic fluid is absolutely necessary to the production of heat, and also that the developement of this elementary principle from bodies, excites a degree of heat proportioned to the quantity extricated in a given time, we are induced to conclude from our general experience of the efficient, a like constancy in all its energies, and by consequence that the evolution of phlogiston, which takes place in the living system is at-

tended with heat. This concluſion is not founded on a few inſtances. It is ſupported by an uniform experience of the regularity of this element, in all its operations: and by experience, as has been already remarked, we chiefly obtain a knowledge of the truths contained in every department of natural ſcience.

We have ſeen from numberleſs phænomena and experiments, that, by whatever means phlogiſton is rapidly, and in any quantity, extricated from the bodies of which it is a conſtituent, heat is uniformly produced. It even appears from Dr. *Franklin*'s thermometrical obſervations, that the evolution of that principle during the vinous ſtage of fermentation, when properly conducted, is accompanied with a degree of heat very nearly the ſame with that of the human body *. The ſimilitude here is by no means remote. The animal proceſs and fermentation are ſo much a-kin, that they ſeem to differ in nothing but the ſubject: For fermentation, in the moſt extenſive acceptation of the word, ſignifies both the ſmalleſt and the greateſt degree of inteſtine motion of the particles of a fluid

* Letters.

abounding

abounding with elaſtick and inflammable matter, from which new combinations are formed.

In this ſenſe, the animal juices muſt at all times be in ſome degree of ferment; for they not only contain a large proportion of the inflammable principle, but a new diſpoſition of their particles is conſtantly taking place. Of this, the great and ſudden changes, produced upon the vital ſtream, give ſufficient evidence. It has been already ſhewn in how ſhort a time the chyle is converted from a white, bland, inſipid fluid, which it is when taken up by the lacteals, into one of a deep red colour. It was likewiſe evinced by incontrovertible experiments, that the difference between the arterial and venous blood, as well as their quick tranſition into each other, depends ſolely on the more or leſs evolved ſtate of their phlogiſton; and the almoſt incredible quantity of that principle thrown off from the ſurface of the body, under the form of perſpiration, and carried off from the lungs by means of the atmoſpherical air, leave no room to doubt of its uninterrupted extrication from the maſs of blood, during the progreſſive change, which that fluid is conſtantly undergoing in the living ſyſtem. In ſhort,

when we attentively reflect on all these circumstances, and recollect that animal heat is by no means high, we can, I apprehend, have little doubt that the cause assigned is adequate to the effect, and sufficient to account for the whole phænomena.

This general conclusion will, I flatter myself, be admitted by every physiologist, who shall attentively and impartially weigh the evidence on which it is founded: At the same time it is not to be expected that, in the rigorous scrutiny to which every new theory must submit, even all its radical principles should at first appear incontrovertible. To obviate, therefore, as far as the nature of things will admit, the difficulties, which the ingenuity of some, and the misapprehensions of others, may suggest, I shall subjoin some short illustrations of such points, as have not been sufficiently explained in the course of this treatise.

The *evolution* and *separation* of phlogiston are words which I have often used, but never yet defined. By the *evolution* of phlogiston, I have all along meant the actual extrication of that principle from those compounds, into which it enters as a constituent part, whether that takes place in consequence of friction, of elective

elective attraction, the introduction of adventitious particles of fire, or be effected in a more gradual and imperceptible manner, in conſequence of a ſpecific inteſtine motion ſuperinduced among the conſtituents of a compound body; as in the caſe of fermentation and the animal proceſs.

By the *ſeparation* of phlogiſton, I have underſtood, either its diſſipation after being diſengaged from the compounds, of which it formed a component part, or its eſcape from bodies unattended with their decompoſition; as in the caſe of ſome precious ſtones, and other phoſphori, which imbibe it from the light of the ſun, and part with it again in the dark, or in any other medium capable of attracting it from them. Such is the meaning I have annexed to the words, *evolution* and *ſeparation* of phlogiſton; and if it do not altogether tally with the common notion of philoſophers, it is to be remembered, that as terms are no more than arbitrary ſigns for things, every writer may uſe them in what ſenſe he chooſes, provided that he explain himſelf.

Another point, which requires ſome elucidation, is this: On what other than mechanical principles can the action of the vaſcular

cular syſtem contribute to the generation of animal heat? It is well known to chymiſts, that no body, whether ſolid or fluid, the conſtituent parts of which maintain a permanent relative connection with one another, is capable either of producing or retaining heat. To the generation and ſupport of an equable degree of heat, it is not only neceſſary, that the ſubject matter be of a heterogeneous and mutable nature, but that it be conſtantly undergoing an actual change, and inceſſantly ſupplied with freſh materials capable of undergoing the ſame mutations. It is on this account, that the moſt celebrated phyſiologiſts, of every age, have conſidered the blood as the grand elaboratory of animal heat. It is a fluid poſſeſſed of every characteriſtick, which chymiſts have found neceſſary to the producing of a uniform degree of heat. It is a mixt, conſiſting of at leaſt three different craſes blended together. It is ever in a progreſſive motion, undergoing conſiderable changes, and it is conſtantly ſupplied with freſh materials naturally fitted for the ſame proceſs. The blood is therefore the ſource of the heat of animals; and we have endeavoured to prove that the immediate circumſtance, on which that phenomenon depends, is

is the developement of the phlogiston which it contains. Hence, though we have reprobated the notion of heat being excited in consequence of the mere mechanical action of the vessels on the vital stream, yet common sense and analogy lead us to believe, that it powerfully promotes that specific intestine motion of the constituents, on which, the transition of the phlogiston from a fixed, to a free and active state immediately depends.

That simple agitation is, on some occasions, sufficient to bring phlogiston into an active state, appears from Mr. *Boyle*'s luminous liquor. This consists of a small quantity of common phosphorus dissolved in spirits of wine, and plentifully diluted with water. Whilst this compound is allowed to remain undisturbed, it is as little luminous as water; but scarcely hath it been shaken for a few moments, when it begins to emit copious streams of light. If then the simple conquassation of a heterogeneous fluid be sufficient to extricate its phlogiston, is it not highly probable, that the strong and unremitting action of the vascular system on the mass of blood, should be a powerful secondary cause in promoting the evolution of the same principle from it.

Whether

Whether is it during the actual evolution of the phlogistic fluid, or after its separation, that heat is produced? That question, which naturally arises from the preceding observations, is perhaps of less importance than some may imagine. The subtile nature of that active element doth not admit of any decisive experiments; but if we be allowed to reason from the phænomena attending its decomposition from bodies, heat is excited the instant that it begins to be extricated, and prior to any certain indications of its separation and dissipation. In the vinous stage of fermentation, there is always a very sensible degree of heat some time ere any inflammable gas can be collected. But though this and several other chymical facts evince the generation of heat during the actual evolution of the phlogistic fluid from the compounds, of which it forms a part, yet, as it unquestionably is the pure and simple element of fire, it is highly probable that it produces heat from the moment it is brought into an active state, till it pass into another mixt, or be entirely dispersed and dissipated. For my purpose, it is enough that its transition from a fettered, to a free state, is accompanied with the sensation of heat.

It

It may with ſome ſhow of probability be objected to my theory in general, that there are inſtances both of the evolution and ſeparation of phlogiſton, in which the thermometer is not ſenſibly affected *. The ruſting of metals, and the putrefaction of vegetable and animal ſubſtances have been inſtanced in ſupport of this. But though theſe be truly phlogiſtic proceſſes, in the firſt inſtance the principle of inflammability is extricated much too ſlowly to have any ſenſible effect; and in the ſecond, beſides that circumſtance, the activity of the phlogiſton is always diminiſhed by the volatile alkali, and other matters, that fly off with it.

When the ſeparation of phlogiſton takes place without decompoſition, heat is rarely produced. We have ſeen that the *Bononian* ſtone imbibes the ſolar light, and yields the ſame coloured rays in the dark. Gems, and moſt kinds of precious ſtones, poſſeſs the ſame property. By rubbing a black piece of

* *N. B.* Chymiſtry furniſhes us with numberleſs inſtances of acids and alkalis meeting without any ſenſible efferveſcence; and yet chymiſts account efferveſcence as univerſal an effect of ſuch mixtures, as I have ſuppoſed the generation of heat, in conſequence of the developement of phlogiſton.

 cloth

cloth, which has been hung in the ſun to dry, we produce innumerous ſcintillations or lucid ſparks. In all thoſe inſtances there is an obvious ſeparation of phlogiſton without heat; but in none of them is it accompanied with any inteſtine motion, change of mixture, or decompoſition. Beſides all theſe phoſphori have their light ſo weak as to ſhine only in the dark; a proof that the quantity of phlogiſton eſcaping from them is extremely ſmall. The caſe is different with thoſe phoſphori, whoſe light depends on their decompoſition. All theſe have unqueſtionably the eſſential property of fire. They uſually, indeed, put on the appearance of innocent lambent flames, but are eaſily ſuſceptible of a burning quality. Thus common phoſphorus is immediately kindled into a moſt ardent and unextinguiſhable flame by common fire. Nay, the evolution of its phlogiſton may be ſo much accelerated without the introduction of adventitious ignifick particles, as to produce heat, and burn: Thus a piece of blue paper, on which we have written with it, will, if ſufficiently rubbed, kindle into an ardent flame, and be immediately conſumed: An irrefragable proof that the ſole reaſon why the evolution of phlogiſton from phoſphoric

bodies

bodies doth not produce heat, is becauſe it is effected in too ſlow and gradual a manner. Chymiſtry furniſhes us with a ſufficient number of facts to infer that the degree of heat generated is always, *cæteris paribus*, in proportion to the more or leſs rapid extrication of the inflammable principle. In actual inflammation, the extrication goes on ſo extremely quickly, that the body is ſoon conſumed, and all the phlogiſton diſſipated. The empyreumatic oils, when not dephlegmated, on being combined with the ſtrong nitrous acid, produce an intenſe degree of heat, without being fired: and in the ſolution of metallic ſubſtances by the mineral acids, the heat is always proportionable to the ſtate and quantity of phlogiſton in the former, and the power of attraction in the latter. In ſhort, from the heat, which accompanies actual inflammation, to that produced by fermentation, we ſhall find every intermediate degree depending on the different degrees of celerity, with which the phlogiſton is extricated.

I am ſenſible that many important queſtions ſtill remain to be aſcertained, on the ſubject of this treatiſe; but as my intention was rather to excite than ſatisfy the curioſity

of phyſiologiſts, I ſhall conclude this chapter with recommending, to their particular attention, that ſingular fluid, on which animal heat hath been ſaid to depend, and which, I doubt not, will be found to conſtitute a much more material part in the living ſyſtem than they in general ſeem aware of. From what has been ſaid on the nature and energies of phlogiſton, it may, I preſume, be juſtly inferred, that it is nothing elſe than the pure ethereal and ſimple element of fire, inherent in all natural bodies, intimately connected or blended with an acid, earthy or other baſe; and apparently, though perhaps not really, remaining therein in a quieſcent ſtate, till excited into action by friction, attraction, adventitious heat, or other ſimilar means. Chymiſts have not been able to procure it in a ſeparate form. They can only diſengage it from bodies by combuſtion, and then it always reſumes the ſtate of pure and active fire, which is well known to be incoercible; or ſeparate it by means of elective attraction, in which caſe it is not indeed reduced to actual fire, but, as the inſtant it quits one body, it becomes a conſtituent of another, we have no acceſs to form an idea of it in a pure ſtate. The impoſſibility of obtaining phlogiſton

giſton in any other ſtate, than that of free and active fire is a convincing proof, that it is really the pure element of fire *. We have numberleſs inſtances in chymiſtry of the moſt active bodies being deprived of their activity, by the union which they contract with the other conſtituents of mixts. Do not the mineral acids, the moſt active bodies in nature after phlogiſton, conſtitute the mildeſt compounds? Is not ſulphur, the vitriolic acid combined with inflammable matter? And nitre, the nitrous acid ſaturated with fixed vegetable alkali? We can therefore have no difficulty in conceiving that the phlogiſtic fluid, though the principle of fire, ſhould be united in ſuch a manner with uninflammable bodies, as to conſtitute compounds, that are neither hot, nor luminous, but capable of producing both light and heat, more or leſs eaſily and copiouſly, according to the more or leſs fettered ſtate of the phlogiſton, and the quantity in which they contain it. In ſhort, there are ſo many phænomena, ſo many facts and obſervations, which concur in evincing phlogiſton to be on all occaſions the proximate cauſe of heat, that the

* Dictionaire de Chimie.

only

only remaining way of evading this concluſion, ſeems to be to adopt at once the ſentiments of a late writer, who abſolutely denies the exiſtence of ſuch a principle in nature *. That author will not, I think, be moleſted in his opinion; its abſurdity will ever be a ſufficient intrenchment to it. It is even with me a matter of doubt, whether the refutation of ſuch a doctrine would not be a greater inſult on the judgment of the learned, than the advancing of it. It is to be regretted, that an ambition for ſingularity ſhould lead men to waſte their time and talents in endeavouring to miſlead others.

All nature bears teſtimony not only to the exiſtence of the phlogiſtic fluid, but likewiſe to its inceſſant active energy. It appears, from innumerable phænomena, to pervade all nature, and by its various modifications to conſtitute the peculiar diſtinguiſhing properties of bodies. The ſolar beams are fraught with phlogiſton. Light and fire conſiſt in it; and whatever influence they have in generating and ſupporting animal and vegetable life, is ſolely to be aſcribed to it. It is to the ſame ethereal fluid, that metals owe their

* Journal de M. Rozier, 1776.

ſplen-

ſplendour, ductility, and elaſticity; magnets their polarity, and all bodies their electricity. It abounds in vegetables; their colour, taſte and ſmell depend on it. In fermentation, efferveſcence, and putrefaction, it eſcapes under the form of a permanently elaſtic vapour; in diſtillation, it is diſengaged and brought over in an ardent inebriating ſpirit, retaining its original properties in a purer baſe. But this ethereal fluid or ſubſtance of light, though preſent in all parts of the earth and firmament, remains latent and unobſerved, till ſome fortuitous occurrence, or exciting cauſe, bring it into action, and render it viſible in its effects. The *Platoniſts* and *Pythagoreans* maintained, that next to the infinite mind and great creative power, which preſides in the mundane ſyſtem, the immediate mechanical or inſtrumental cauſe, that moves and actuates all its parts, is the pure element of fire; in ſhort, that it is the material ſpirit or ſoul of the univerſe, and that as it actuates the macrocoſm, ſo it animates the microcoſm. Such was the real doctrine of theſe ancient ſages: Nor did they widely err. When we look around us with a philoſophic eye, and contemplate with ſedulous attention univerſal nature, we are convinced

vinced that there is no effect great, marvellous, or terrible, but proceeds from fire. From a too contracted view of the laws of nature, that diffused and active principle has been distinguished by almost as many names as energies: but we have shewn that it is the same powerful agent which we at one time contemplate in the form of concentrated light tearing asunder the densest adamant, and at another rending the clouds, and threatening with resistless destruction the lofty oak and towered citadel. And, with respect to the microcosm, it is highly probable that it is phlogiston in its pure electric state, that actuates the vital frame, and is the immediate physical cause of motion, the source of sensation, of irritability, and sympathy; in a word, that it is what hath been generally denominated by physiologists the animal spirit, or nervous fluid. Is it not in favour of this opinion that paralytic limbs frequently recover sense and motion by means of electricity? And doth not the connexion, which invariably subsists betwixt the degree of heat and the state of the living principle in animals, tend to shew that they both depend on the same cause, *viz.* the state of phlogiston? The warmest animals are likewise the

the most perfect, and those animals, whose temperature hardly exceeds that of the element in which they live, seem almost destitute of every function peculiar to animated bodies. In fine, if we reflect that nothing nourishes, but what contains phlogiston in a state readily separable by the animal process; and that this principle is, without interruption, evolved and reduced to an active state by the powers of circulation, we shall find every reason for believing that it is the chief pabulum of animal life, the grand principle of muscular motion, and the only source of heat in living animals.

Some may perhaps find it difficult to conceive how so many various phænomena, as are here attributed to the agency of phlogiston, should depend on any single elementary principle. I acknowledge the plausibility of the objection, but appeal to philosophers in general, if it be not easier to conceive that one agent should produce many effects, than that many agents should, without confusion, exert at the same time, and on the same body their respective powers, as of electricity, gravity, magnetism, elasticity, volatility, *&c.* Besides, have we not innumerable instances, in the history of nature,

 of

of the ſame agent producing various effects? Doth not fire excite heat, cauſe light, extenſion, elaſticity? Doth not the electrick fluid produce the effects of attraction, repulſion, magnetiſm, light, heat, *&c.*? Can we have more convincing proofs that a fluid, whoſe parts are actuated by different motions, may produce effects ſo diſſimilar, that we at firſt ſhould ſuppoſe them to depend on cauſes eſſentially different? We ought likewiſe to reflect, that all the properties, which we perceive in the bodies around us, are more or leſs obviouſly related to each other. The knowledge, or the diſcovery of thoſe relations, is almoſt the higheſt pitch to which the imbecillity of the human underſtanding permits us to attain, and the ſole object, by conſequence, to which our inquiries ought to be turned. Vague, and arbitrary hypotheſes will never unveil to us the myſteries of nature. It is only by an attentive and careful inveſtigation of phænomena, by accurately collating them, by judiciouſly marking their points of ſimilitude and coincidence, by the art, in ſhort, of reducing many to one, which may be conſidered as the ſource, and principle of all, that we may reaſonably expect to advance in the ſcience of nature. It is thus we

we have corrected the chimerical notions of the ancients, reſpecting the nature of heat. They believed it to differ in different ſubjects, in kind, as well as in degree. They ſpoke magnificently of the celeſtial heat, and ſuppoſed it very different from that produced on the terraqueous globe; but a due attention to phænomena has taught us, that all heat, whether of an elementary, igneous, or animal kind, is of one and the ſame nature.

CHAP. VI.

The most striking Phænomena of Animal Heat evince the Truth of the Theory proposed.

IN the investigation of difficult subjects the analytic method ought ever to precede the synthetic; I mean, that we should begin with the phænomena, or effects, and from them investigate the powers, or causes, that operate in nature; and being once possessed of these, we should then descend in a contrary order, and from them, as established principles, explain all the phænomena that are their consequences, and prove our theory. Unless that truly genuine method of philosophizing be pursued, we can never be certain that we assume the principles, which really obtain in nature, and that our system, after we have composed it with great labour, is not a mere dream and illusion. Hitherto all our inquiries have been directed to the discovery of the cause, on which, the heat of animals

animals depends; and now we ſhall endeavour to ſhew that the chief phænomena of animal heat readily admit of a ſolution on the principles I have been labouring to eſtabliſh. There is nothing to be concluded with reſpect to the probability of a phyſical hypotheſis, before we know the number of phænomena which it is deſigned to explain, and have appreciated the accuracy of its explications; for as the true cauſe is ſufficient to explain all the phænomena, and to explain them alſo in all circumſtances, it is evident that the hypotheſis will approach to the true cauſe in proportion to the number it explains, and to the degree of ſucceſs with which it explains them. I therefore enter upon this, though not the leaſt difficult part of my ſubject, with alacrity, as I hope to ſhew that the theory propoſed will afford a ſatisfactory explanation of many phænomena inexplicable on former hypotheſes. I ſhould, however, far exceed the limits of my preſent deſign, were I to mention all the peculiarities of animal heat, and on that account I ſhall content myſelf with treating, in a curſory manner, of its more remarkable attributes.

SECT.

SECT. I.

The Connection of Animal Heat with the State of Motion in the ſanguiferous Syſtem.

THE ſtate of heat, in living animals, appears to be ſo intimately connected with the condition of the vaſcular ſyſtem, that the greater part of phyſiologiſts have not heſitated to conſider the mutual attrition of the veſſels, and the circulating maſs of blood, as the ſole cauſe of the heat of animals. They were led into this opinion, by obſerving that an increaſe of heat always attends a briſk circulation, and that a languid, or interrupted circulation, is always accompanied with a proportional diminution of it. But, without repeating the arguments, which I formerly brought to evince the impoſſibility of animal heat being generated in the coarſe mechanical way, in which they wiſh to explain it, I ſhall only ſhow that every inſtance, that ſeems to countenance their hypotheſis, tends ſtill more obviouſly to illuſtrate and corroborate mine.

The

The two chief instances, on which they ground their hypothesis, are the cases of violent exercise, and a total stoppage of circulation. In the former, the increased velocity of the pulse is no doubt attended with a proportional increase of heat; in the second, the vital fluid no sooner begins to stagnate in its canals, than a gangrene supervenes, and the heat of the part sinks below the natural standard. Both these phænomena, however, may be easily explained without having recourse to the mechanical powers of friction and attrition. All, who are acquainted with the laws of the animal economy, know, that the motion of the blood depends immediately upon the action of the containing vessels; and many cogent arguments have been brought, to shew that the developement of its phlogiston is in a great measure effectuated by the action of the circulating powers: whence it evidently follows, that the more phlogiston *(cæteris paribus)* there is evolved, the more heat will be generated. This is sufficiently confirmed by the quantity of phlogistic matter observed to fly off under the form of perspiration in febrile disorders, and violent exercise. It is also conformable to this opinion, that a diminution of heat should

attend

attend an interrupted circulation; for ſince the evolution of the phlogiſtic fluid depends upon the action of the veſſels, whatever aboliſhes this, muſt prevent that, and therefore no heat will be produced.

Several medical writers of conſiderable fame relate caſes, in which a full and accelerated pulſe was attended by a ſenſible degree of cold, and a languid one by an evident increaſe of heat. In all ſuch inſtances, however, I am inclined to ſuſpect, that phyſicians have truſted more to the feelings of the patient, than to accurate thermometrical obſervations; and the caſe of a man (whom *De Haen* mentions) who complained of the moſt intolerable cold, when a thermometer ſhewed the heat of his body to be eleven degrees above the natural ſtandard, demonſtrates how very little we can depend upon our ſenſes *. I do not, however, mean to affirm that there are no exceptions to the general principles I have laid down; but I will venture to ſay, that they are much more rare than phyſicians ſeem to imagine. This may ſafely be inferred from ſuch caſes having ſeldom, if ever, occurred ſince thermoſcopical obſerva-

* Rat. Medendi. *Vol.* II.

tions

tions became more general. We have indeed many instances, where no increase of heat attended a celerity of pulse, but not a single well vouched instance, in which the heat of the body was increased without a quickened circulation. A mere sensation of heat is not meant, but a heat real and sensible to the thermometer; nay, it would almost be absurd to suppose it, as the preternatural increase of heat, however excited, would soon become a *stimulus* to the vessels, and accelerate their action; and therefore the motion of the blood, whether as a cause, or effect, is necessarily connected with the state of heat in the animal. Celerity of pulse unattended with an increase of heat, is a fact, which, I apprehend, we shall find no difficulty in explaining.

1mo. It should be observed that an accelerated pulse doth not always indicate an augmented action of the sanguiferous system: on the contrary, it seldom fails to occur, where the action of the heart and arteries is much diminished. This fact is sufficiently demonstrated by Dr. *Hales* *. In one of his experiments to ascertain the projectile force of the

* Statical Essays. *Vol.* I.

heart, he fixed a glaſs tube to the crural artery of a horſe, when his pulſe beat forty times only in a minute; but the pulſe, as the horſe became fainter, was gradually more and more accelerated, till at laſt it exceeded a hundred pulſations in a minute. Whence we learn that as the powers of circulation diminiſh, the quickneſs of the pulſe increaſes. This, however, is no argument againſt the truth of our theory, as it was ſhewn that the developement of phlogiſton depends upon the powers of circulation; and therefore the increaſe of heat ſhould be in the compound ratio of the velocity and momentum of the blood.

2*do*. Though the evolution of phlogiſton depends chiefly on the influence of the circulating powers, yet from the view we took of the different degrees of heat, in the different claſſes of animals, we learned that it was very intimately connected with the ſtate of their fluids; and hence it may be inferred that the condition of the blood in the ſame animal, may at different times be more or leſs fitted for the extrication of phlogiſton; and this ſuppoſition, a variety of circumſtances tend to countenance. It was formerly obſerved that the fluids of the fœtus are perfectly

fectly bland, and on chymical analysis afford only a very small portion of the principle of inflammability. If then it be manifest that the fluids of the fœtus contain less phlogiston than those of the adult, we can hardly doubt that circumstances frequently occur, which cause the quantity present in the fluids to vary at different times, through every stage of life. A total abstinence from animal food, and all those alimentary substances, in which this principle abounds; a debilitated state of the digestive powers; a leuco-phlegmatick or dropsical habit; in a word, whatever affects chylification must contribute somewhat either to prevent a due quantity of phlogiston from being taken into the body at all, or render its evolution more slow even when present in the general mass.

From what hath been just now said of the state of the fluids in the fœtus, we are enabled to explain a phenomenon, which appeared to Dr. *Priestley*, and other experimental philosophers, a problem of no easy solution, *viz.* that young animals live much longer in a given quantity of air, than old ones. The most convincing proofs were brought that the chief purpose of respiration is to carry off phlogisticated matter from the

lungs; and thence it follows, that, as in young animals a smaller quantity thereof is explicated from the blood, the air is not so soon saturated with it, and on that account, they are able to subsist for a longer period without a fresh supply, than older animals. This simple and natural explanation of the matter tends strongly to corroborate my theory of respiration, and leads me to take notice of an opinion, which Dr. *Monro* delivers in his anatomical lectures on this subject.

That celebrated physiologist from finding that fishes soon die in water exhausted of its air, concludes that besides *somewhat* noxious to the animal being washed off and carried away, there is likewise *something* useful taken in, but as neither he, nor any other philosopher, have yet attempted to explain the nature of this supposed *pabulum vitæ*, we are at liberty to deny its existence entirely, especially as this, and the other facts, on which the hypothesis is founded, admit of a more simple explanation. It appears from many accurate experiments, that pure inflammable air by standing in water loses its inflammability, and becomes again fit for respiration. This fact shows how fishes can live without the atmospheric air. Agitation, we know, powerfully

powerfully promotes the union of phlogiston with water. How wonderfully then are the gills of fishes adapted to this purpose, since the constant and alternate admission and expulsion of water by them, far exceed the most ingenious contrivances of our experimental philosophers for impregnating water with phlogisticated air; and it is sufficiently evident from the gills being of a much deeper red, than any other part of the fish, that no inconsiderable quantity of phlogiston is in reality carried off by them. It is however to be remarked, that though we can impregnate water with this principle, yet various experiments shew that its attraction for phlogiston very much depends on the quantity of air it contains; and therefore, when the air is exhausted, the affinity is diminished, and a breathing animal can no more live in it, than a flame *in vacuo* *.

SECT.

* Chymistry doth not furnish us with a single instance of a direct combination of phlogiston with water; I mean, of a body which consists merely of it and water. But the principle of inflammability is capable of being united to compounds of which water is a constituent: Such are spirituous, oily, and saline substances. It is entirely owing to its phlogiston that sulphur

SECT. II.

Stability of Animal Heat.

One of the moſt ſtriking phænomena of the heat of living animals, is the power with which they are endowed, of preſerving nearly the ſame degree of heat, in all the variations of the temperature of the air. The vital principle of animals ſeems to depend upon a certain degree of heat; and this degree, in all the more perfect animals of every age, ſex, and conſtitution, vibrates from 96 to 98 degrees of *Fahrenheit*'s thermometer; and while life and health remain, it continues the ſame in ſummer as in winter, in the frigid as in the torrid zone. I ſhall begin by inveſti-

phur is not ſoluble in water. The difficulty of combining this principle with water, without an intermediate ſubſtance, is common to all other mixtures in which it abounds in a ſtrongly attracting ſtate. Vapour may not improperly be ſaid to conſiſt of fire and water, but there is here no intimate union of parts, for without a conſtant ſupply of adventitious heat, the vapour is immediately condenſed: but of this, more hereafter.

gating

gating the powers, by which animated body is enabled to resist degrees of heat much superiour to its natural temperature.

This subject hath of late very much engaged the attention of the ingenious in different parts of the world. In France, *du Hamel*, *Tillet* *, in Britain, *Fordyce*, *Blagden*, and *Dobson* have instituted many ingenious and interesting experiments, with a view to ascertain the extent of this power, which seems so universally to attend life ‡. But tho' it appears from the general result of these experiments that animals can, with impunity, support such high degrees of heat as in the days of *Boerhaave* would have been thought absolutely incompatible with life, yet there is reason to regret that they have not thrown so much light upon the subject as might naturally have been expected. For after weighing, with the most painful attention, the inferences, which these gentlemen have deduced from their several experiments, we shall find, I am afraid, that the cause of this singular phenomenon is as much as ever a *desideratum* in physiology. Dr. *Blagden*, indeed,

* Mem. de l'Acad. des Sciences.

‡ Phil. Transact. *vol.* LXVI.

aſſures us that his experiments prove, in the cleareſt manner, that the animal body hath a power of producing or deſtroying heat, juſt as the circumſtances of the ſituation require; and that it is a power of ſuch a nature, as can only be referred to the principle of life, and probably exerted in thoſe parts of our body, in which life ſeems peculiarly to reſide. I hope, however, to make it appear that the matter is by no means ſo very clear, as he ſeems to imagine; and on the contrary, that concluſions, widely different from thoſe he has drawn, are deducible from his experiments. In order to evince the truth of this aſſertion, it will not be neceſſary to repeat the objections formerly made to the *Cullenian* doctrine of animal heat, on which Dr. *Blagden*'s is founded. To ſay that cold or heat is eſſential to life, or that life can generate either, independent of chymical or mechanical means, is, as the *London* Reviewers very juſtly obſerve, "proſtituting the uſe of words, "and recurring to occult qualities to account "*abſurdly* for things, becauſe we have not "yet made ſufficient diſcoveries to account "for them *philoſophically*." That we may, in this ſcrutinous inquiry, with more certainty,

ty, preclude every chance of fallacy, no advantage ſhall be taken of any theory hitherto advanced on the ſubject of animal heat, but the argument ſhall reſt ſolely, on the more general laws of heat, and on the experiments, as related by Dr. *Blagden* himſelf. I have likewiſe, that we may be enabled to form a more accurate judgment of the foundation, on which he grounds his poſition, ſelected an experiment from each of the papers publiſhed by him in the Philoſophical Tranſactions, and given them in his own words ||.

EXPERIMENT I*.

ABOUT three hours after breakfaſt Dr. *Fordyce* having taken off all his cloaths, except his ſhirt, in the third room, and being furniſhed with wooden ſhoes, tied on with

|| Vol. LXV.

* *N. B.* Theſe experiments were made in a *ſuite* of rooms heated by flues in the floor, and by pouring upon it boiling water. There was no chimney in them, nor any vent for the air, excepting through crevices at the door. In the firſt room were placed three thermometers; one in the hoteſt part of it, another in the cooleſt part, and a third on the table, to be uſed occaſionally in the courſe of the experiment.

 liſt,

liſt, entered into the ſecond room, and ſtaid five minutes in a heat of 90°, when he begun to ſweat gently. He then entered the firſt room, and ſtood in the part heated to 110°; in about half a minute, his ſhirt became ſo wet that he was obliged to throw it aſide, and then the water poured down in ſtreams over his whole body. Having remained ten minutes in this heat of 110°, he removed to the part of the room heated to 120°, and after ſtaying there twenty minutes, he found that the thermometer placed under his tongue, and held in his hand, ſtood juſt at 100°, and that his urine was of the ſame temperature; his pulſe had gradually riſen to 145 pulſations in a minute. The external circulation was greatly encreaſed, the veins had become very large, and an univerſal redneſs had diffuſed itſelf over the body, attended with a ſtrong feeling of heat; his reſpiration however was little affected. He concluded this experiment in the ſecond room, by plunging into water heated to 100°, and, after being wiped dry, was carried home in a chair; but the circulation did not ſubſide for two hours, after which he walked out in the open air, and ſcarcely felt the cold.

EXPE-

EXPERIMENT II.

"I (Dr. *Blagden*) took off my coat, waistcoat, and shirt, and in that situation went into the room, as soon as the thermometer had risen above the boiling point (212°). The first impression of the air upon my body was much more disagreeable than I had ever felt through my cloaths, but in five or six minutes a profuse sweat broke out, and took off all my extraordinary uneasiness: at the end of twelve minutes when the thermometer had risen to 220°, I left the room very much fatigued, but no otherwise disordered: my pulse beat 136 pulsations in a minute *."

Thus the experiments are related, and I appeal to every unbiassed philosopher, whether there be a single circumstance in either of them, which marks a power, I mean, a positive and active power, peculiar to the

* *N. B.* We are told by Dr. *Blagden*, that in some other experiments on the same subject, Dr. *Fordyce* and he had found, that even the 260th degree of *Fahrenheit's* thermometer could be submitted to with tolerable ease. My only reason for selecting the two experiments here recited, was their being stated in fewer words than any of the others.

 animal

animal economy, of reſiſting or deſtroying heat. For my part, I do not heſitate to affirm, that the bodies of thoſe gentlemen would have attained to a degree of heat equal to the ſurrounding medium (ſuppoſing ſuch a degree compatible with life), in as ſhort a time, as an equal bulk of the moſt inert and lifeleſs matter of a ſtructure anywiſe ſimilar to the animal machine, I mean, of a ſoft, ſpungy, looſe texture, and equally well fitted for evaporation.

It is ſomewhat remarkable that a man of Dr. *Blagden*'s penetration and accuracy, ſhould have overlooked ſo many material circumſtances in eſtimating the quantity of heat communicated to bodies in a given time. He neither adverts to the quantity of matter in his body, nor the lowneſs of its temperature, on his entering the room, and ſeems to have entirely forgot that the ſlowneſs, or celerity, with which heat is communicated, very much depends on the nature of the medium, thro' which it paſſes, and the nature of the ſubſtance receiving it. For it is not only notorious that rare bodies are the ſloweſt conductors of heat, and that air, the medium in his experiments, is the rareſt of all fluids, but likewiſe that the time, which bodies take in

coming

coming to an æquilibrium of heat, depends leſs on the real, or apparent quantity of matter which they contain, than on certain circumſtances in their aggregation fitting them to receive heat: But in order to throw ſome light on this ſubject, I ſhall make a few curſory obſervations on the more general laws of heat.

One of the moſt remarkable properties of heat is its tendency to flow from warmer to colder bodies, till an æquilibrium be produced between them. This ſo univerſally takes place, that it is impoſſible to make heat remain, for any length of time, in any maſs of matter placed without the ſphere of activity of a poſitive cauſe of heat, and deſtitute of any intrinſick principle capable of producing it. To account for the difference of time that different bodies take in attaining the temperature of the ambient medium, hath been long a ſubject of ſpeculation among philoſophers; but, at preſent, we ſhall proſecute it no farther than is neceſſary to explain the experiments before us.

Muſſchenbroeck is of opinion that fire is equally diſtributed through equal ſpaces, and that there is as much of it in a cubical inch of air, as in a cubical inch of gold; and his

reaſon

reaſon for thinking ſo is, that the thermometer points to the ſame degree in both. In this particular, however, that ingenious chymiſt is egregiouſly miſtaken; for heat is to be conſidered both in reſpect of its degree, and its quantity, as appears from the following ſimple experiment. If we expoſe a cubical inch of wood, and a cubical inch of iron, for the ſame ſpace of time, to the ſame degree of heat, and then immerſe them into equal quantities of cold water, we ſhall find that the water, into which the iron was plunged, ſhall riſe many degrees higher than that into which the wood was thrown. It is therefore demonſtrably true, that iron, when heated, containeth a greater quantity of heat than the ſame bulk of wood; and that wood requires at leaſt more time, if not a greater quantity of heat than iron, to bring it to the ſame temperature. This led Sir *Iſaac Newton*, and other philoſophers, to believe that heat is in proportion to the quantity of matter in bodies. This opinion, however plauſible it may appear in theory, doth not accord with facts and experience. Dr. *Martinè*, in order to aſcertain the facility with which bodies heat and cool, put equal quantities of quickſilver and water into two phials herme-

hermetically ſealed, and found, on immerſing them into boiling water, that the mercury received heat much faſter than the water, and in greater quantity, but loſt it much ſooner, notwithſtanding its ſuperior denſity *.

It is therefore evident, that the temperature of ſome bodies is much ſooner raiſed, and with much leſs heat, than that of others; and I am convinced that this circumſtance, which ſeems to have been overlooked by the major part of experimental philoſophers, hath been the ſource of much errour and deception. The vaſt latitude, which different bodies ſhew with reſpect to their capability of receiving heat, tends to corroborate the arguments formerly adduced to prove that fire is not merely an intenſe vibratory motion of the integrant particles of bodies, and in the compound ratio of the velocity and denſity of the matter, but is really a material and an active fluid capable of pervading every ſubſtance in nature, and of exhibiting, in certain circumſtances, the phænomena of heat. According to the mechanical ſyſtem, the denſeſt bodies ſhould give out the greateſt

* On Thermometers.

quantity

quantity of heat, but this is evidently not the caſe, for if water and lead be heated to the ſame point, two cubick inches of water will produce as much heat in a given quantity of cold water, as five cubick inches of lead.

Nothing hath ſo much contributed to the improvement of our ideas of heat, as the invention of thermometers. They are the only teſts by which we can judge, with certainty, of the length of time different bodies take to heat and cool, and the proportions of heat, which they receive, or loſe, in a given time, when expoſed to equal quantities of heating or cooling cauſes. Sir *Iſaac Newton* ſuppoſed that the progreſſion with which heat is received, or loſt, is geometrical; and Dr. *Martine* imagines that it is a compound of geometrical and arithmetical progreſſion; but farther experiments ſeem neceſſary to determine the diſpute. The queſtion of greateſt importance, and which hath puzzled chymiſts moſt, is to aſcertain the real principles, on which the heating and cooling of bodies immediately depend.

Dr. *Boerhaave* has laboured to prove that bodies are more ſlowly, or quickly altered by the heating or cooling influence of the ambient medium, in proportion to their denſity; and

and this is no doubt a plausible speculation, and what we should naturally deduce from the *vis inertiæ* of matter: but it is a position, by no means conformable to the abovementioned experiments on quicksilver and water. The celerity, with which heat is communicated from one body to another, depends on a variety of circumstances. A chief circumstance is the extent of contact and close communication of parts; for when bodies are composed of the same quality and quantity of matter, their becoming hot or cold will be in due proportion to the extent of surface. This is the reason why a cube cools faster than a sphere of the same metal *. The distribution of heat is more or less rapid in proportion to the closeness of contact among the insensible particles of mixts. This is obvious, from the celerity, with which it passes into hard compact bodies, and the excessive slowness, with which it is imparted to substances of a loose, rare and spungy texture.

With respect to fluids, the reason of their conceiving heat so fast seems to arise entirely from their expansibility ‖. When a vessel

* Newton. Princip. Matth.

‖ Dr. *Black's* Lectures on Chymistry.

 with

with water is placed over the fire, the heat expands and renders lighter what is next the bottom, which of courſe aſcends to the top, while another part, more cool and denſe, deſcends to the bottom. Thus there is a perpetual circulation from the upper to the lower part of the veſſel. It is on theſe principles that we explain the ſeverity of winter froſts in extenſive continents, and their mildneſs in ſmall iſlands. When a cold wind blows over an extenſive and deep ſea, the ſuperficial water muſt be immediately cooled to a certain degree, and the wind proportionably warmed. But the ſuperficial and cold water becoming ſpecifically heavier than the water below it, deſcends; what is warmer ſupplies its place, and this change of the ſuperficial water, and ſucceſſive aſcent of that, which is warmer, and conſequent ſucceſſive abatement of coldneſs in the air, will go on till the whole water be ſo far cooled as to be at laſt arreſted by froſt.

Were it not for the tranſparency of the air, and the expanſion, which it ſuffers, as it is the rareſt of all fluids, it would be of all bodies the ſloweſt conductor of heat. That this is the caſe in a given quantity of air appears from the following experiment.

If

If we take three vessels of equal capacity, and fill the first with air, the second with water, and the third with quicksilver, all of one temperature, and in each suspend a piece of iron heated to the same degree, we shall find that the iron will cool extremely slowly in the vessel replete with air, much sooner in the water, and still faster in the mercury. The cooling of the iron in this experiment is, however, far from being in the inverse ratio of the density of the different fluids, as *Boerhaave* alleges *. The reason, why heat flows faster into dense than rare bodies, into which, *a priori*, it might be supposed to penetrate with greater facility, is still a problem in physicks. All the conjectures hitherto offered are insufficient to satisfy a rational inquirer, though each hath had its vogue and day. The following observations will, I hope, throw some light on the subject; and when an investigation is from its nature so intricate and obscure, that we can hardly expect to arrive at conclusions which are certain, there may be some utility in pointing out such as are probable.

* Elem. Chym.

The temperature of the air becomes colder and colder the higher we aſcend. All thoſe, who have viſited Mount *Ætna*, have experienced the truth of this poſition. That mountain is divided into three diſtinct regions, which are as different in climate and productions, as the zones of the earth, and perhaps, with equal propriety, might have been ſtyled the torrid, the temperate, and the frigid zone. When the harveſt is quite over, and the heat inſupportable at the foot of the mountain, at *Nicoloſi*, which is only twelve miles up it, the corn is ſtill green, and the heat moderate. In the ſecond region, the heat is ſeldom obſerved to riſe above 60 degrees in the hotter ſummer months: and near the ſummit of the mountain, which is covered with ſnow perhaps coeval with itſelf, the temperature of the air is always at leaſt five degrees below the point of congelation. Every circumſtance mentioned, with reſpect to the difference of climate on Mount *Ætna*, is equally applicable to the *Andes*. Theſe mountains, no leſs remarkable for extent than elevation, may be literally ſaid to hide their heads in the clouds. The ſtorms often roll, and the thunder burſts, below their ſummits, which, though expoſed to the rays of the ſun

ſun in the center of the torrid zone, are covered with everlaſting ſnows. The ſhowers of hail, which fall in ſummer, are a farther proof of the coldneſs of the ſuperior regions of the atmoſphere. This is a curious and intereſting fact, and if the principles, on which I am about to explain it, are well founded, they will clear up many difficulties reſpecting the unequal diſtribution of heat.

The ſun is, no doubt, the ſole poſitive and permanent ſource of heat, as well as of light, on the ſurface of the terraqueous globe; but various circumſtances, beſide the direct force of his rays, tend to regulate the heat, which he communicates to the atmoſphere. Experience hath taught us that he imparts little or no heat to ſuch bodies, as are perfectly tranſparent. If a burning glaſs be ſo placed, that the focus falls a little below the ſurface of a tranſparent fluid, it doth not heat the fluid. A convex lens, while it produces a degree of heat intenſe enough to vitrify flint, does not ſenſibly affect a thermometer applied to it: nay, a lens may be conſtructed of ice, powerful enough to inflame wood, and fuſe metals. It is therefore ſufficiently manifeſt that the rays of light do not heat a diaphanous medium, and that, to produce any re-

markable

markable effects of heat, they must be modified, and variously reflected, by opaque bodies. This is the reason, why the superiour regions of the air, which are so much more rare and transparent than the lower, are likewise so much colder; and were it not for the solid heterogeneous bodies floating in it, I question if it would be at all susceptible of heat. Now the atmosphere, which environs the summits of those lofty mountains covered with eternal snows, is so extremely rare and free of every kind of earthy particles floating in it, that the solar rays meet with nothing to reflect them; and according to Sir *Isaac Newton*, they generate no heat in a medium through which they pass undisturbed, in right lines. "Quod radii solis non agitant "media quæ permeant, nisi in reflectione & "refractione." Upon the whole, it appears that the heat produced is always in proportion to the number of solar rays collected and reflected by opaque bodies.

But though we have explained the cause, why the rays of light do not heat a transparent medium, no philosopher, as far as I know, has been able to account in a satisfactory manner, why bodies receive heat, *cæteris paribus*, in proportion to their density.

For

For my own part, after considering the question with a good deal of attention, I must acknowledge that I have nothing certain to offer on it, and even suspect that a satisfactory explanation exceeds the limits of our present physical knowledge. Conjectures, however, may be admitted, which as they are not intended to decide, do not establish or confirm errours, but may be useful in suggesting hints to future inquirers. A thousand different phænomena teach us, that bodies absorb the heat of the sun, in proportion to their opacity; and experiments, without number, shew that culinary heat is communicated to bodies in proportion to their density; and both solar and artificial heat depend upon one and the same cause, the phlogistick fluid. This analogy between the *modus agendi* of solar and culinary heat, at one time appeared to me sufficient to throw considerable light on the subject; but notwithstanding its being a well established maxim in phylosophizing, that similar effects can only proceed from similar causes, I shall leave it to future experience to determine whether dense bodies receive heat faster, and in greater quantity, than rare ones, on the same principles, that opaque substances absorb the solar heat, whilst

whilſt pellucid bodies retain only a very ſmall portion of it. It was formerly ſhewn that the electrical matter is really a modification of phlogiſton, and numberleſs obſervations ſhew that the worſt conductors of electricity are, in general, the ſloweſt conductors of heat. A comparative view, founded on experiment, of the conducting power in bodies of heat and electricity would, I am perſuaded, throw no inconſiderable light on both thoſe important phyſical queſtions; and this, I would ſuggeſt as a proper ſubject of inquiry to ſuch as have more genius and leiſure to proſecute it, than I am poſſeſſed of. The foregoing obſervations on the more general laws of the diſtribution of heat among bodies will, I apprehend, enable us to explain, whence it happens that the animal body may, in certain circumſtances, be expoſed to very high degrees of heat, without ſuffering any conſiderable change in its natural temperature.

The chief circumſtances to be adverted to in Dr. *Blagden*'s experiments are, 1*ſt*. the degree of heat to which the rooms were heated; and the quantity of matter in his own and the other gentlemen's bodies. 2*d*. The medium, through which the heat was tranſmitted;

ted; and *lastly*, The texture of the human body, and its particular tendency to spontaneous evaporation.

1st. It would certainly have astonished the great *Boerhaave*, to have been assured by several philosophers of undoubted veracity, that they had for many minutes remained, with impunity, in a medium, in which the thermometer had risen several degrees above the temperature of boiling water, when he, in consequence of his own and *Fahrenheit*'s experiments, had pronounced it impossible for man to subsist in a degree of heat, in which the mercury rose above that of his own body. But the *Boerhaavian* doctrine, in this particular, hath been long since proved erroneous. Mr. *Tillet*, a French Academician, was the first, who instituted a course of experiments to point out the fallacy of those, on which *Boerhaave* had grounded his opinion *. *Du Hamel*, and others, have since followed his example ‡. but I have pitched on Dr. *Blagden*'s, both on account of their being more generally known, and because he hath drawn conclusions from them, which, in my apprehension, are by no means consonant with sound philoso-

* Mem. de l'Acad. des Sciences.

‡ *Ibidem.*

 phy,

phy. How far I am right in this aſſertion, can only be determined after a full diſcuſſion of the matter.

The firſt thing to be conſidered is the degree of heat, to which the thermometer roſe in the heated rooms. This was indeed ſo extremely high, that at firſt, it almoſt appears incompatible with animal life; but when we have coolly and maturely weighed all the concomitant circumſtances, our aſtoniſhment, I truſt, will entirely ceaſe, and we ſhall then comprehend how the human body can bear with impunity, for a ſhort ſpace of time, the contact of air, in which the thermometer hath riſen above 260 degrees. It appears, from the above-mentioned experiments of Dr. *Martine*, that the temperature of quickſilver is more quickly raiſed, and with a leſs quantity of heat than that of any other fluid, with which we are yet acquainted. And as all the thermometers uſed in Dr. *Blagden*'s experiments were made with quickſilver, it is evident that they would receive heat much faſter, and in greater quantity, than his body, or any other ſubſtance that was not of the ſame denſity and tenacity as mercury. Of this Dr. *Blagden* himſelf furniſhes us with an irrefragable evidence. He tells

us

us that " an earthen vessel, containing pure " water, being placed in the heated room, " it was a full hour and a half before it rose " to *gr.* 140, and never came near the boil- " ing point, but continued stationary above " an hour at a much lower degree *." Can we desire a more convincing proof of the different fitness in inert matter of receiving heat, than that a small quantity of water should be an hour and a half in acquiring 140 degrees, whilst the mercury in the thermometer rose in a few minutes to *gr.* 204. Since so small a quantity of water was such a length of time in arriving at this low degree of heat, it plainly follows that a considerable body of it would have, in the same space of time, suffered very little change in its temperature; nay, I venture to affirm that if a volume of quicksilver, equal in bulk to Dr. *Blagden*'s body, notwithstanding its extreme facility of being heated, and its incapacity of spontaneous evaporation, had been exposed for the same space of time to the same degree of heat, it too would have exhibited strong marks of a *power of generating cold*, and been found, at the expiration of twelve minutes (the time he remained in the heated

* Philosoph. Transf. *vol.* LXV.

room),

room), almoſt as far from an æquilibrium of heat with the ſurrounding medium, as his body. It is therefore manifeſt, that no concluſions can with any certainty be deduced from experiments of this nature, without exactly aſcertaining the quality and quantity of matter in the reſpective bodies; their comparative extent of ſurface, and the ſpecific difference of their aptneſs to receive and tranſmit heat: for unleſs thoſe points be duly attended to, no general concluſions can on juſt grounds be inferred.

2do. The next circumſtance to be conſidered is the medium, through which the heat was tranſmitted to their bodies. We have ſeen that the ſlowneſs, or celerity, with which heat is communicated to bodies, depends in a great meaſure on the nature of the conducting medium; and facts and obſervations have taught us that bodies convey heat quickly, or ſlowly, according as their texture is more or leſs denſe, and hence the rareſt bodies are the ſloweſt conductors of heat. Now, both in Dr. *Fordyce*'s and Dr. *Blagden*'s experiments, the conducting medium was as rare, as we can well imagine: In the former, it was common air ſlightly loaded with a watery vapour: And in the ſecond, pure dry air,

air, highly rarefied by heat. And how much the communication of heat depends upon the medium through which it is transmitted, Dr. *Dobson*'s late ingenious experiments sufficiently evince. "Part of the shell "of an egg (says that judicious philosopher), "was peeled away, leaving only the film "which surrounds the white; and part of "the white being drawn out, the film sunk "so as to form a little cup. This cup was "filled with some of the *albumen ovi*, which "was consequently detached as much as possible from every thing but the contact of "the air and the film which formed the "cup. The lower part of the egg stood "upon some light tow in a common galley-"pot, and was placed in a wooden seat in "the stove. The quicksilver in the thermometer still continued at 224 degrees after remaining in the stove for an hour; the "lower part of the egg which was covered "with the shell, was firmly coagulated, but "what was in the little cup was fluid and "transparent. At the end of another hour "it was still fluid except on the edges, and "there it was still transparent, a sufficient "proof that it was dried, not coagulated *."

‡ Phil. Transact. *vol.* LXV.

This

This experiment, which I have recited in Dr. *Dobſon*'s own words, affords a ſtriking inſtance of the unequal diſtribution of heat, and proves, I think, to demonſtration, that very much depends on the nature of the conducting medium. By what other principles can we explain whence the *albumen ovi*, which in water coagulates at 150 degrees, ſhould, when not in contact with its own ſhell, or any denſe body, remain fluid when the thermometer has reached to *gr*. 224? Is it not in favour of the ſame opinion that the human body, when naked, can bear without immediate injury, an atmoſphere *ſeemingly* heated to 260 degrees; whereas in the ſame medium, if covered with tin, it would quickly experience an intolerable and deſtructive degree of heat? I have ſaid *ſeemingly* heated to *gr*. 260, for were the temperature of the ſurrounding medium really and poſitively ſo high, no animal whatever (of ſalamanders I write not) could poſſibly ſubſiſt for a few minutes; nay, I ſhould juſt as ſoon believe that Dr. *Blagden* had remained ten minutes without any inconveniency in a cauldron of boiling water, as in air abſolutely heated to 260 degrees; and yet neither he, nor Dr. *Dobſon*, ſeem to have entertained the ſmalleſt

ſmalleſt ſuſpicion that the air, in the heated rooms, was of a different temperature from that indicated by the thermometer. It was the overlooking of this very material and leading circumſtance that rendered their deductions ſo egregiouſly erroneous. Had theſe gentlemen reflected on the more general laws of heat, they would no doubt have inferred, that its diſtribution in their experiments muſt be the ſame, as on all ſimilar occaſions, and by conſequence that the heat of the ambient medium, to that of the thermometer, would be nearly as the tenacity and denſity of quickſilver are to that of air.

It may, perhaps, at firſt appear a little paradoxical to aſſert that the air ſhould have tranſmitted to the thermometer a much greater quantity of heat, than it was really poſſeſſed of; but a thouſand known facts and experiments warrant this deduction. Have we not ſeen that the ſuperiour regions of the atmoſphere, through which the ſolar heat is communicated to the terraqueous globe, increaſe in coldneſs, in proportion to their diſtance from the earth; and does not every experimenter know, that a piece of wood at the bottom of a bowl of water may, by means of a burning-glaſs, be reduced to char-

coal

coal without heating the intervening fluid. We may now, therefore, affirm what we formerly alleged, that the ſolar heat acts ſolely on opaque bodies, and culinary heat on denſe ones. Whoſoever contemplates the nature and properties of pure atmoſpheric air, muſt, I think, be convinced that it is ſuſceptible only of a very ſmall degree of heat: The immediate and uniform effect of heat upon air is the producing of a proportional rarefaction thereof; and ſuch is its power of elaſticity, that no ſubſtance in nature can confine it in a condenſed ſtate, when heat is applied. It is therefore obvious to remark, that ſince bodies the rarer they are, the leſs they are fitted to receive heat, the fluid air, whoſe rarefaction increaſes in proportion to the heat applied, and whoſe elaſticity renders it incoercible, cannot poſſibly be heated to any high degree. By not attending duly to this circumſtance, many ingenious philoſophers have been exceedingly miſled in their experimental inquiries.

Dr. *Dobſon* has ſubjoined to his experiments, ſeveral judicious obſervations and curious ſpeculations, on the ſubject of heat: There is however one phenomenon, which he has been extremely puzzled to explain; and

and it is, how the animal body can bear with impunity the contact of air heated to 260 degrees (for he had no doubt of the air and thermometer being of one temperature), whilst, if covered with tin, it would soon perish at a much lower degree of heat? In order to solve the problem, he proposes it as a query, whether, in the last instance, the fire does not pass into the body in a sensible and active state, whilst in the first, it becomes immediately latent and quiescent? But had the case really stood as he imagined, we should in vain, I suspect, have attempted to expound it on the principles of latent heat.

Dr. *Black*, amongst the other important discoveries, which he hath made in natural philosophy, has proved by incontrovertible experiments, that heat, on some occasions, becomes latent in bodies. The difference between latent and obvious heat is, that the former doth not affect the most sensible thermometer, whilst the latter is so far in a fluctuating state, that it uniformly passes from a hotter to a colder body, till an æquilibrium between them be produced. But when heat becomes latent, it evidently doth not depend on the manner, in which it is communicated. Dr. *Black* found that ice, before it became

fluid, abſorbed 147 degrees of heat; but this equally happened, whether it was allowed to diſſolve ſlowly in warm air, or was ſuddenly melted in warm water *. Another objection to Dr. *Dobſon*'s ſuppoſition is, that ſenſible heat is never obſerved to become latent, or quieſcent, unleſs when bodies are either paſſing from a ſolid to a fluid ſtate, or converted into vapour.

It is therefore ſufficiently plain, that there is no reaſon to believe that bodies receive heat in an active, or a quieſcent ſtate, according to the mode in which it is applied to them. Dr *Dobſon* came much nearer the truth, when he attributed the unequal diſtribution of heat in his experiments, to the difference in the nature of the medium, through which it was tranſmitted. This deduction is not only ſupported by his own experiments, but is likewiſe ſeconded by ſeveral particulars in thoſe of Dr. *Blagden*. This gentleman informs us, that the pieces of metal, which he had with him in the heated rooms, felt intolerably hot, whilſt the air, from which they had derived their heat, was only unpleaſant: and adds, that the ſame perſon, who ſuffered

* Chym. Lectures.

little

little inconvenience from air, in which the thermometer rose to 260 degrees, could not bear quickfilver at 120 degrees, and could just bear rectified spirit of wine at 130. All these facts concur in shewing, that bodies receive heat faster and in greater quantity, in proportion to their density; and that they impart it, quicker or slower, on the same principles. Had Dr. *Blagden* duly adverted to these several circumstances, he would not have expressed so much astonishment on finding that the human body was capable of supporting, with impunity, a degree of heat, which, after many hours, could not bring a small quantity of water near the boiling point, nor even coagulate a small portion of *albumen ovi*, when not in contact with a dense body.

3tio. The last thing to be considered in Dr. *Blagden*'s experiments, is the texture of the human body, and its fitness for receiving heat *ab extra.* It appeared, from the principles formerly explored, that heat is communicated to bodies faster or slower nearly in proportion to their density. It is therefore manifest that the human body, which is plainly of a loose, rare, and spungy texture, would receive heat extremely slowly, and in small

quantity; particularly, when the medium was ſo very rare as in the experiments before us. But the chief circumſtance which enables man, and other analogous animals, to reſiſt external heat, is the tendency of the economy to ſpontaneous evaporation; the powerful effects of which, in diminiſhing heat, however explained, admit of no diſpute.

It is well known to phyſiologiſts, that all over the external ſurface of the body, and throughout the whole expanſion of the cuticle, there is a perpetual tranſudation of the recrementitious fluids, eſcaping under the form of a ſubtile vapour. When this exhalation is copious enough to become an object of the ſenſes, as in ſweat, it is ſtyled *ſenſible* perſpiration; when it is ſo ſubtile as to eſcape their notice, it is ſaid to be *inſenſible*. The quantity of matter evaporated in this way, appears, from the moſt accurate ſtatical experiments, to exceed all that is evacuated by the other emunctories *. *Sanctorius*, the celebrated *Paduan* phyſician, found that in *Italy*, under the circumſtances of a temperate diet, middle age, and an eaſy life, the matter inſenſi-

* *Keil* Med. Stat. Brit.

bly

bly perspired was equal to 5-eighths of the whole *assumpta*. Friction, violent exercise, external heat, and whatever quickens the circulation, and raises the pulse, promote the evaporation, as by this means a greater quantity of perspirable matter is, in a given time, brought to the exhaling vessels. "Quodcunque cursum et circulum sanguinis promovet, perspirationem augere certissimum est *." It is therefore apparent that the exhalation, which takes place from every pore of the human body, though very considerable at all times, may still be much increased by external causes.

The effect of evaporating fluids in producing cold, seems to have been observed, about the same time, by M. *de Mairan* in *France* ‡, and Mr. *Richman* at *Petersburg* ‖; but nothing has thrown so much light on the subject, as Dr. *Cullen's* ingenious experiments, published some years ago in the *Edinburgh* physical and literary Essays. From several of these it appears, that the refrigerating power of evaporation is very great. In one instance,

* *Hoffman.* med. rat. Syst.

‡ Dissertation sur la Glace.

‖ Nov. Comment. Petropol. 1747-8.

when

when the heat of the air was about 53 degrees, he ſet a veſſel containing nitrous æther, in another, a little larger, containing water, and placing both under the exhauſted receiver of an air pump, he found, in a very few minutes, moſt part of the water frozen, and the veſſel containing the æther ſurrounded with a thick and firm cruſt of ice *. I have repeatedly ſatisfied myſelf of the ſudden diminution of heat, in conſequence of evaporation, by moiſtening with ſpirit of wine the ball of a thermometer; and I have always obſerved, that the quickſilver ſunk faſter, or ſlower, in proportion to the quickneſs of evaporation.

N. B. Mr. *Nairne*, in the Philoſophical Tranſactions for 1777, mentions an experiment which he made, to try whether he could produce any conſiderable degree of cold, by the evaporation of æther under a receiver, whilſt exhauſting; and found that when the degree of exhauſtion, by the barometer gage was 65, the degree of cold indicated by the fall of the thermometer, was 48, below 0 in *Fahrenheit*'s ſcale; ſo that there was a degree of cold produced 103° colder than the air of the room, where the experiment was made; the thermometer in it being at 55 degrees above 0. The air being let into the receiver, the remaining æther was examined, and there were found ſeveral pieces of ice at the bottom of the phial, ſome of them as big as large peas.

Various

Various solutions have been given of the cause of spontaneous evaporation; but that which supposes it, on all occasions, to depend upon heat, seems liable to the fewest difficulties. It is obvious that vapour becomes more copious as the heat increases, and that it condenses again, when the heat is removed. It is on these principles only, that we can rationally account for the phænomena of mists, dews, *&c.*; but though we may thence infer that spontaneous evaporation is very analogous to the production of an elastic steam, in a fluid that has attained the boiling point of heat, yet we have still to investigate the cause, on which, its power of producing cold immediately depends. Several conjectures have been offered on the subject. Some have endeavoured to explain the phenomenon, by ascribing it to the effect of the mixture of the evaporating fluids with the air; but that this is not the case, is sufficiently apparent from Dr. *Cullen's* experiments, which clearly prove that the cold produced depends more on the volatility of the aggregate, than on the nature of the mixt; and that the evaporation *in vacuo* is accompanied with a greater degree of cold, than in the open air. The most satisfactory solution of this curious physical problem has, in

in my opinion, been given by Dr. *Black.* That ingenious philoſopher, as was formerly hinted, was led by a variety of circumſtances to conclude, that on ſome occaſions, a quantity of obvious heat becomes latent; and on others, that latent heat emerges, and becomes obvious. After water is brought to the boiling point, it ſhould, in a few minutes, fly all off in vapour; but inſtead of this, it is conſtantly receiving heat from the fire, which heat is neither ſenſible in the water, which continues at the ſame degree, nor in the vapour, which is not hotter than the water; but on condenſing this vapour, a prodigious quantity of heat becomes obvious, which was before concealed in a latent ſtate. Dr. *Black*, and Dr. *Irwin*, found upon accurate calculation, by obſerving the refrigeratory of a common ſtill, that the heat of the vapour, if not in a latent ſtate, muſt have been equal to 800 degrees.

The arguments, which ſhow that on certain occaſions latent heat aſſumes a ſenſible form, are not leſs concluſive than thoſe, which evince heat being abſorbed and becoming latent. A multitude of facts and obſervations tend to prove, that obvious heat never becomes latent in ſolid bodies, till they arrive

arrive at their melting point. Fluidity is ſo univerſal an effect of heat, that there is hardly a ſubſtance in nature, which either alone, or compounded, may not be fuſed by means of burning mirrors: we even find that not only ſolid bodies can be rendered fluid by heat alone, but likewiſe that thoſe bodies, to which fluidity has been thought eſſential, on having their temperature ſufficiently diminiſhed, aſſume a ſolid form. Profeſſor *Braun* of *Peterſburg* *, by a mixture of the nitrous acid and ſnow, congealed quickſilver ſo firmly, that it required ſeveral ſtrokes of a hammer to reſtore its fluidity ‡.

The degree of heat neceſſary to produce fluidity is extremely different in different bodies, but is uniform and ſtable in the ſame. It does not however depend on ſenſible heat alone. We find that as long as a body continues ſolid, heat, if increaſed, is perceptible

* Nov. Comm. Petropolit.

‡ Mr. *Hutchins* informs us, that at *Albany*, in *Hudſon's Bay*, in 1775, by means of a mixture of melting ſnow and *ſp. nitri fumans Glauberi*, the quickſilver in his thermometer was ſo completely frozen, that after breaking the tube, he repeatedly ſtruck the cake with a hammer, and heard it give a deadiſh ſound like lead. *Phil. Tranſ.* vol. LXVI.

by the thermometer; but the inſtant it arrives at the melting point, the thermometer becomes ſtationary how long ſoever we continue to throw heat into it. Dr. *Irwin* of *Glaſgow*, found that a quantity of melted tin thrown into water, raiſed its temperature in a much greater proportion, than a ſolid maſs of the ſame metal heated, as nearly as poſſible, to the ſame degree: an irrefragable proof that the heat, which continues to flow into bodies, after reaching their melting point, is not loſt by communication, as philoſophers have in general ſuppoſed, but that it is abſorbed and remains quieſcent in them, till they be cooled down to a certain degree; after which, if they be placed near to colder bodies, it begins to emerge and gradually reſume a ſenſible form: and it is only when the latent heat is entirely gone, that the body returns to its ſolid ſtate, and acquires the temperature of the ſurrounding medium. I have repeatedly found, on expoſing to the open air, when ſeveral degrees below the freezing point, a decanter filled with water, in which a thermometer was ſuſpended, that the quickſilver fell to 27°, whilſt the water remained perfectly fluid; but that the inſtant the cryſtals began to encompaſs the ball of the

the thermometer, it roſe to 32°. M. *de Mairan* and Dr. *Black* relate ſeveral experiments, in which they obſerved the water to fall many degrees below the point of congelation, but that upon freezing, it always became warmer, and roſe to 32°. We can hardly, I think, doubt that the heat, which emerged on theſe occaſions, was really preſent in the water in a quieſcent ſtate, and was the cauſe of its fluidity. For if the converſion of water into ice depended ſolely on the diminution of ſenſible heat, it ſhould be all frozen in a few minutes after coming to the freezing point: but this, we have ſeen, is not the caſe. There is in the Philoſophical Tranſactions of laſt year, a courſe of experiments on the freezing of ſea-water, which, though intended by their ingenious author, Mr. *Nairne*, to reſolve a different phyſical queſtion, throw, in my opinion, conſiderable light on the ſubject of latent heat. As they are too numerous to be here inſerted, I muſt beg leave to refer the reader to the original paper *.

But though Dr. *Black* has, I think, proved beyond diſpute that ſenſible heat be-

* Vol. LXVI.

comes latent during the tranſition of bodies into vapour, yet he has not, as far as I know, attempted to explain the principles on which this curious phenomenon depends: and indeed ſo low and imperfect is, to this day, the ſtate of our knowledge of the operations of nature, that, notwithſtanding the boaſted diſcoveries of the preſent age, hints and conjectures are in reality all that we in general have to offer in lieu of ſatisfactory ſolutions of phyſical problems. This confeſſion, however, is the leſs humiliating, that the vanity of man prompts him to hope that what he now throws out, as ſimple conjecture, will one day be eſtabliſhed on the firm baſis of obſervation and experiment: How far this may be the fate of many ſpeculations hazarded in the courſe of this treatiſe, time alone can determine. But to return to my ſubject. Since it is chiefly during the rarefaction of water into vapour that obvious heat becomes latent; and during the condenſation of vapour to its former ſtate that latent heat emerges and becomes ſenſible, is there not reaſon to believe that the whole is explicable on electrical principles. Water, in its natural denſe ſtate, is incapable of being charged with more electricity than it already poſſeſſes; but, when

rarefied

rarefied into vapour, is capable of attracting and retaining a much greater proportion of electrick matter. "When a portion of wa-"ter (says Dr. *Franklin*) is in its common "dense state, it will hold no more electrick "fire than it has: When the same portion "of water is rarefied into vapour, and forms "a cloud, it is then capable of receiving and "absorbing a much greater quantity, there "is room for each particle to have an electrick "atmosphere." Now, as various arguments were formerly adduced, not only to evince the unity and identity of the electrick matter and phlogiston, but likewise to prove that the latter is the true elementary principle of fire, is there not a strong presumption that during the transition of dense fluids into vapour, the adventitious and super-abundant phlogiston forms an electrick atmosphere round each minute particle of vapour, and continues in that state till, by the approach of colder bodies and the consequent change of temperature, the vapour returns to its condensed state; and as it is then no longer capable of retaining the same quantity of electrick fire, this is again separated, under that form of action in which it produces heat. This hypothesis is the more probable, that

that the difference in the electrick ſtate of the ſame bodies, at different times, very much depends on their temperature.

This ſhort account of the theory of latent heat, will, I hope, enable us to explain the power of evaporating fluids in producing cold. We have every reaſon to believe that vapour is produced in the ſame manner as fluidity; and conſequently, that a quantity of ſenſible heat becomes quieſcent in it, as Dr. *Franklin* chooſes to ſpeak, and continues in that ſtate till the vapour begin to be again condenſed. The vaporifick point of fluids, though it plainly depends on a certain degree of heat, is widely different in different bodies, but is uniformly the ſame, in the ſame fluid, in ſimilar circumſtances. The terms *volatility* and *fixedneſs* are only terms of compariſon; thoſe bodies being ſtyled *volatile*, which require leſs heat to convert them into vapour, than moſt others, and *vice verſa.* The chief circumſtances, which ſeem to influence ſpontaneous evaporation, are, 1*mo.* Mechanical preſſure; 2*do.* The degree of heat applied; and 3*tio.* The moiſt or dry ſtate of the atmoſphere.

1*mo.* Mr. *Boyle* found that ſpring water, which under the preſſure of the atmoſphere requires

requires 212 degrees of heat to bring it to the boiling point, boils in the exhausted receiver at the temperature of the human blood: and vitriolick æther, whose boiling point in air is 96°, boils *in vacuo* below the freezing point of water. Various other instances might be adduced to evince the powerful influence, which mechanical pressure hath over the evaporation of fluids. In *Papin*'s digester, water may be heated to 400 degrees above the boiling point, without being all converted into vapour.

2do. Heat seems to promote spontaneous evaporation in two ways; both by its action upon the fluid, and on the ambient air. In the first place we know that heat augments the repellent power in the particles of fluids, and by thus increasing their elasticity, causes them to expand into a larger space, and assume the form of vapour. And secondly, as the density of the air on the earth's surface varies with its temperature, and that its pressure is equipollent to its density, it is evident that the rarefaction of the air will be according to the degree of heat applied, and by consequence its pressure on the evaporating fluids, proportionally diminished.

3tio. The

3*tio*. The third circumſtance, which we have ſuppoſed to influence evaporation, is the moiſt or dry ſtate of the atmoſphere. Whether we adopt Lord *Kaims*'s ingenious theory of evaporation, as publiſhed by Mr. *Hamilton* of *Dublin*, and conſider air as a real ſolvent of water, or that, with M. *de Luc*, we ſuppoſe the phenomenon to depend ſolely upon heat, we muſt, in either caſe, allow that air is capable only of keeping a certain quantity of water ſuſpended in it; as is apparent from its condenſation into dew, miſts, and rain: and hence the more vapour the atmoſphere is already loaded with, ſo much the leſs will it carry off in a given time, and *vice verſa*. Theſe are the chief external circumſtances, which tend to increaſe, or diminiſh, the natural tendency of bodies to ſpontaneous evaporation; and they will be found to have more or leſs influence, according to the degree of volatility peculiar to the evaporating fluids. Thus the nitrous æther is entirely converted into vapour, at more than 100 degrees below the freezing point of water. Several other things contribute more or leſs to evaporation, ſuch as extent of ſurface, agitation, &c. which are too obvious to require any explanation.

From

From theſe obſervations, we are enabled to account for all the phænomena, which attend the evaporation of fluids. We learn from them, that a ſudden and intenſe cold is produced *in vacuo*, in conſequence of the quickneſs of evaporation, when not influenced by the preſſure of the atmoſphere; that the ſinking of the thermometer is greateſt, when the air, in which the experiment is made, is warm and dry, becauſe heat increaſes the elaſticity of fluids, and diminiſhes the preſſure of the air: and laſtly, that the power of evaporating fluids in producing cold, is nearly according to the degree of volatility in each; becauſe the more volatile a body is, the faſter, and with the leſs heat, is it expanded into vapour.

Since therefore animal perſpiration, and ſpontaneous evaporation, are unqueſtionably one and the ſame proceſs, we may reaſonably ſuppoſe them regulated by the ſame law, and productive of the ſame effects; and conſequently that the more perſpiration is augmented, the more ſenſible heat becomes latent; or in other words, the more cold is produced. Why then call in the aid of a power of deſtroying heat in the animal body, of whoſe exiſtence we have no proof, and of

whoſe *modus agendi* we have no concep-tion? Has not nature, by framing the living ſyſtem, in ſuch a manner as to render it in an eminent degree capable of evaporation, ſuffi-ciently provided againſt the viciſſitudes of the atmoſphere, in every ſeaſon, and in every climate? Obſervation and experiment have taught us that, between the tropicks, the tendency of the human body to evapora-tion increaſes in the ſame proportion as it diminiſhes in the neighbourhood of the poles. Dr. *Blagden* himſelf is obliged to allow that, in his experiments, the living powers were very much aſſiſted by perſpiration, though by no means enough to account for the whole of the cooling; but had he reflected that as the heat increaſed, ſo did the ſpontaneous evaporation from the ſurface of his body, and that the unuſual degree of fatigue, which he felt after leaving the rooms, could only pro-ceed from the more violent effort of the cir-culating powers to increaſe both ſenſible and inſenſible perſpiration; had Dr. *Blagden*, I ſay, adverted to theſe, and the other circum-ſtances formerly mentioned, he muſt have been convinced that the cooling effects of evaporation are ſufficient to account for all the phænomena; and that it is in reality the great

great agent employed by nature in preserving the just balance of temperature in the animal machine.

With respect to Dr. *Fordyce*'s experiments in moist air, which Dr. *Blagden* supposes to evince the contrary of the above conclusions, it must be observed in the first place, that the degree of heat was by no means so considerable as in his own; and secondly, the highly rarefied vapour, with which the air was impregnated, would have very little, if any effect, in preventing an increase of perspiration proportioned to the degree of heat. For though we have said that a dry state of the atmosphere is more favourable to the evaporation of fluids, than a moist one, yet as nothing so much relaxes the simple solids of animals, as heat and moisture combined, and that nothing favours perspiration so much as relaxation, it is plain that the vapour, with which the air was fraught, in Dr. *Fordyce's* experiments, would rather promote, than obstruct perspiration. This to me is the true *modus operandi* of the vapour-bath, and the only principles on which its salubrious effects are to be explained. A cold moisture applied to the surface of the body no doubt checks perspiration; but that is not only owing to

 the

the air being already ſaturated with a watery exhalation, but more eſpecially to the tonick power of the cold, which braces the ſolids, and conſtricts the cutaneous pores. Beſides, cold water is by no means ſo good a ſolvent of perſpirable matter as hot, and therefore cannot carry it off ſo quickly from the open mouths of the exhaling veſſels; and ſhould it ſtill be urged that Dr. *Fordyce* was incapable of ſupporting ſuch high degrees of heat in moiſt as in dry air, we ſhall find no difficulty in accounting for it, on the difference in the denſity of theſe two bodies. We have ſeen that bodies are ſuſceptible of heat nearly according to their denſity; if therefore it be conſidered that water is 860 times denſer than air, and that when it boils, it has acquired all the heat of which it is ſuſceptible in an open veſſel, the concluſion muſt be, that air will boil much ſooner than water; that it will acquire, in a much leſs compaſs of time, all the heat of which it is ſuſceptible, and hence that the moſt ſubtile watery exhalation will communicate more heat than air, which has been expoſed to the ſame heating cauſe. The contrary of this opinion was ſtrenuouſly ſupported by M. *Deſaguliers*, who wrote a very ingenious paper to prove that water, by evaporation,

evaporation, is rarefied in a greater proportion than air; but reiterated experiments have ſhewn his calcul to be extremely erroneous; for water, when expanded into ſteam, inſtead of 14000 times, as he alleges, is only increaſed in bulk 1660 times. Many other arguments might be brought to ſhew that the reaſon why Dr. *Fordyce* could not remain ſo long in moiſt, as in dry air, though ſeemingly of an inferior temperature, was entirely on account of the different denſities of the medium; vapour being ſuſceptible of a much greater degree of heat, than highly rarefied air.

Theſe are the animadverſions which occurred to me, after attentively weighing every circumſtance in Dr. *Blagden*'s experiments. I have dwelt the longer upon them, becauſe they afforded me an opportunity of enlarging upon ſeveral particulars, which tend to explain and corroborate my general doctrine of animal heat. I ſhall therefore only add that till Dr. *Blagden*, and thoſe who have embraced his opinion, bring more concluſive arguments, and leſs ambiguous experiments, to prove that the living ſyſtem is endowed with an active and poſitive power, or principle peculiar to life, of deſtroying heat, I muſt be permitted to conſider his hypotheſis, not only

as

as a baſeleſs fabrick, but in ſeveral reſpects inconſiſtent with ſound philoſophy. At the ſame time, I deem both Dr. *Fordyce* and Dr. *Blagden* juſtly entitled to the higheſt commendations and applauſe, for the active ſpirit of inquiry, and the endeavours, which they have exerted to extend, by experiment, the ſtill very limited boundaries of every branch of natural ſcience, particularly of the phyſiology of animated body. But ſuch is the fallibility of the human mind, ſo various are its views and apprehenſions, that the gradations, by which it deviates from truth into errour, are almoſt imperceptible; and hence ariſeth the difficulty, which even the moſt profound philoſophers meet with, in laying down a ſyſtem complete and irreprehenſible in all its parts.

Ουδεις ανθρωπων εϛιν απαντα σοφος *

Said an ancient ſage; and it is obvious, that we form our idea of a man's parts, and philoſophical knowledge, not from his having never erred, but from his having erred leſs frequently than others.

* Theognides.

After

After explaining by what means the animal body is capable of ſupporting ſo high degrees of heat, as thoſe mentioned in the foregoing experiments, we ſhall, I preſume, find little difficulty in accounting for the ſtability of animal heat, under the natural viciſſitudes of external temperature in different ſeaſons and climates.

Nature, not content with having endowed man with the tranſcending faculty of reaſon, and of thereby eſtabliſhing him lord of the creation, has, with equal ſagacity and beneficence, caſt his corporeal frame in a more perfect mold than that of other animals. For whilſt theſe are either confined to a particular region of the globe, or if they ſeem capable of exiſting beyond it, gradually dwindle and degenerate from the vigour and perfection peculiar to their ſpecies; man is bleſſed with a frame, which is at once ſo hardy, and ſo flexible, that he can ſpread over the whole earth, become the inhabitant of every region, and multiply under every climate. Now the chief circumſtance, on which the flexibility of the human body, in this reſpect, ſeems to depend, is the power which the economy hath of preſerving uniformity

uniformity of temperature, in the various degrees of heat and cold to which it is exposed.

But though in different regions of the earth, the temperature of the air varies very considerably, we are now from observation and experiment, sufficiently convinced, that the ancients were egregiously mistaken on this head. Of the five zones, into which they divided the terraqueous globe, the two, on which they bestowed the appellation of *temperate*, were alone, according to their notions, allotted to man for his habitation, and the only indeed in which life could subsist. This wild opinion was not a conceit of the uninformed vulgar, or a fanciful fiction of the poets, but a system adopted by the most enlightened philosophers, the most accurate historians and geographers in *Greece* and *Rome*. " There are (says *Pliny*), five divisions of the earth, which are called *zones*. All that part of the earth, which lies near to the two opposite poles, is oppressed with vehement cold and eternal frost. There, unblest with the aspect of milder stars, perpetual darkness reigns, or at most a feeble light reflected from surrounding snows. The middle of the earth, in which is the orbit of the sun,

ſun, is ſcorched and burnt up with flames and fiery vapour; between theſe horrid diſtricts, lie the two other portions of the earth, which are temperate, but on account of the burning region interpoſed, there can be no communication between them *." According to this theory, thoſe fertile and populous regions within the torrid zone, which are now not only known to yield their own inhabitants the neceſſaries and comforts of life, with moſt luxuriant profuſion, but to communicate their ſuperfluous ſtores to the reſt of the world, were ſuppoſed to be the manſion of perpetual ſterility and deſolation. As it would be foreign to my plan to inveſtigate the cauſes, which regulate the diſtribution of heat in different parts of the globe, I ſhall only obſerve that we cannot determine preciſely the portion of heat felt in any region of the earth, merely by meaſuring its diſtance from the equator. In the memoirs of the *French* academy we find ſeveral curious thermometrical obſervations, which ſhew that the ordinary heats, even under the line, are not greater than what have been in ſome ſeaſons obſerved at *Paris*, and other known

* Lib. II.

temperate places in Europe *. But admitting that the temperature of the atmoſphere ſuffers, which it certainly doth, very conſiderable variations, in different climates, we readily perceive a multitude of circumſtances, which conſpire to diminiſh the active power of heat in warm climates, and obviate the deleterious effects of the intenſe cold of northern regions. Upon whatever principles phyſiologiſts have explained the action of external heat upon the animal machine, they ſeem all to agree that it ſtimulates the living principle, augments the irritability of the moving fibres, relaxes the ſimple ſolid, and conſequently diminiſhes the tone of the vaſcular ſyſtem. Now as, I truſt, it hath been evinced, not only that the evolution of phlogiſton is the cauſe of heat in animals, but likewiſe that this evolution depends, in a great meaſure, on the velocity and momentum of the circulating fluids, it is no leſs plain that the quantity evolved will be in proportion to the vigour or debility of the powers of circulation, than that the ſtate of theſe powers will be according to the tonick or relaxing cauſes acting upon them. It ſeems,

* L'An. 1738.

therefore, allowable to affirm, that the generating power of heat diminiſhes as the external temperature riſeth. The ſame inference is deducible from another known law in the animal economy; its tendency to perſpiration. It hath been already ſhewn that ſpontaneous evaporation produces very intenſe degrees of cold, and that its effects are always proportioned to the quickneſs of the proceſs, and the quantity evaporated; and ſince the animal body is ſo framed, that whatſoever quickens the circulation, determines a greater quantity of perſpirable matter to the exhaling veſſels, in a given time, it is apparent, that as the external heat increaſes, the action of the heart and arteries will be accelerated, by which means the ſpontaneous evaporation will become more copious, and a proportional diminution of ſenſible heat take place.

Several other arguments might be deduced from the theory, on which I have explained animal heat, that tend to account for its ſtability. According to the principles formerly laid down, the heat of animals depends upon the condition of the blood. It is therefore obvious to remark, that all circumſtances tending to preſerve the ſtability of the vital

fluid, will preſerve a uniform temperature: and this we find is effected by many contrivances in the ſyſtem, particularly by the ſtate of ſecretions. Till the blood arrive at a certain condition, it is not a proper ſubject for excrementitious ſecretion; but when it reaches that ſtate, it is immediately thrown off: whence it enſues, that as long as there is no morbid affection in the body, the generating power will remain the ſame, and an equable temperature be ſupported, whether the diſcharge into the circumambient air be greater, or ſmaller: but, in diſeaſe, both the generating power and the ſtate of ſecretions are changed, and the uniformity of temperature in conſequence deſtroyed. With reſpect to febrile heat, it very evidently proceeds from the increaſed action of the veſſels, and the ſpaſm on the ſurface of the body obſtructing ſpontaneous evaporation.

If the heat of animals really depends on the developement of phlogiſton, and that the only ſource, from which they can derive this principle, is the aliment, there can be no doubt that the temperature will be influenced by the nature and quantity of the food. By attending to this circumſtance, we are enabled more eaſily to explain the ſtability of heat

heat in different regions of the earth. The quantity of food men consume varies according to the temperature of the climate, in which they live, the degree of activity which they exert, and the natural vigour of their constitution. Under the enervating heat of the torrid zone, and when men pass their days in indolence and ease, they require less nourishment, than the active inhabitants of temperate, or cold climates; and accordingly in these sultry regions, their food consists chiefly of vegetables, which are as well known, from experience, to afford less nutriment than animal substances, as they are from chymical analysis, to contain a much smaller proportion of phlogiston. We therefore perceive two different causes combined in retarding the developement of that principle from the mass of blood; the relaxing power of heat, by which the vigour of the circulation is diminished: and the small quantity of food men consume, besides, what they do take being of such a nature as to contain little phlogiston in a separable state. There are a variety of other circumstances, each of which has more or less effect, in enabling the inhabitants of warm climates to endure the heat, to which they are exposed.

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Their clothing is light and thin; they carefully avoid violent exercife during the more fultry part of the day; the relaxed ftate of their folids renders them extremely prone to fpontaneous evaporation; and fuch is the texture of animated body, that heat is communicated to it extremely flowly through fo rare a medium as air. In fine, whofoever will allow all thefe circumftances their proper weight and influence, will have little difficulty in conceiving how the human body preferves its temperature equable, and unaltered, though expofed to the unremitting ardours of the torrid zone.

SECT III.

Of the Stability of Animal Heat in cold Climates.

Having fhewn by what means living animals preferve, in a medium exceeding their own temperature, a uniform degree of heat, it remains to be explained, how they are capable of refifting degrees of cold below their natural ftandard. It appears, from a courfe

of experiments, instituted by a celebrated physiologist, Mr. *J. Hunter*, with a view to satisfy himself, whether animals retain life after they are frozen; that no animal can be frozen, till it be deprived of every principle of life; and that the power, with which animals resist cold, is in proportion to their degree of perfection, and to the natural heat peculiar to each species *. The result of these experiments so perfectly coincides with the principles I have all along been wishing to establish, that it is just what, *a priori*, I should have expected. From what was formerly advanced respecting the different degrees of heat, in different classes of animals, we readily perceive why it required the greatest cold, which Mr. *Hunter* could produce, to overcome the generating power of heat in dormice, whilst this power, in the toad and snail, was exhausted by a much less degree of cold. The natural heat of these, on account of the paucity of red particles in their blood, the languid state of their circulation, and the small quantity of phlogiston evolved, is not only extremely low, but liable to be soon affected, and materially altered, by ex-

* Philosoph. Transf. *vol.* LXVI.

ternal

ternal circumſtances; and this, in a due proportion to the degree of perfection peculiar to each. Hence, we perceive why, in Mr. *Hunter*'s experiments, the power of generating heat was found, in the toad, ſo much weaker than in the dormouſe, and ſtill weaker in the ſnail, than in the toad. Many and various are the means, which enable the more perfect animals to ſupport degrees of cold conſiderably below their own temperature: but before I proceed to enumerate them, it will not be improper to premiſe a few curſory obſervations, on the more general effects of cold upon the living ſyſtem. Cold, applied to animal matter, ſeems to act in three diſtinct ways, according to its degree and mode of application: 1*mo.* As a ſedative: 2*do.* As a ſtimulant: 3*tio.* As an aſtringent and tonick.

1*mo.* It is manifeſt that a certain degree of heat is abſolutely neceſſary to the ſupport of the vital principle of animals, and that therefore cold, which very plainly conſiſts in the diminution of heat, hath a tendency to deſtroy life. When an intenſe degree of cold is applied to any member of the body, it ſeems to operate by aboliſhing the tone of the veſſels of the part: Firſt, thoſe in immediate contact

contact with the scarf-skin, and, by degrees, those situated more internally; and as the generating power of heat depends upon the state of the circulation, and this upon the tone of the vessels, it is evident that the vital fluid will no sooner begin to stagnate in its canals, than the part will be cooled down to the mean temperature, between 98 degrees and that of the surrounding medium. This opinion is perfectly conformable to the result of Mr. *Hunter*'s experiments, which likewise tend to shew that cold acts by destroying the tone of the vessels, and not by first cooling the part so as to congeal the fluids, as has been generally supposed by physiologists. Facts are still wanted to determine either how great a part of the animal machine, or how far the whole may be diminished in temperature, without the loss of life; but from the recovery of drowned persons, and the case of the dormant animals, it seems highly probable, that both may take place to a considerable degree.

We cannot, however, doubt of the sedative power of cold, when we reflect on the many unfortunate persons, who, in the more northern regions, perish in the snow. On these occasions, the vital principle is over-

 come,

come, the tone of the ſanguiferous ſyſtem deſtroyed, and by degrees the circulation ſtopped. Here every ſymptom beſpeaks the ſedative power of cold. Torpor, debility, and faintneſs, firſt ſeize the helpleſs victim worn out with fruitleſs ſtruggles; then, in ſucceſſive order, follow inſenſibility and coma, which terminate the melancholy ſcene *.

2do. When cold is not intenſe enough to deſtroy the nervous ſyſtem, it then proves a real ſtimulus, either topical, or general. It is well known that the tranſient application of a ſlight degree of cold conſtantly produces a redneſs and heat of the ſkin. This influx of blood into the part, clearly ſhews, that cold acts as a ſtimulus on the ſanguiferous ſyſtem; and as it is probable that cold acts primarily as a ſedative, we ought no doubt to explain its ſtimulant effects on the ſame principles, as thoſe of all other ſedatives, and aſcribe them to the re-action it produces. But how-

* ———————— on every nerve
The deadly winter ſeizes, ſhuts up ſenſe,
And o'er his ſtrongeſt vitals creeping cold
Lays him along the ſnows a ſtiffen'd corſe,
Unſtretch'd and bleaching in the northern blaſt.
THOMSON.

however this may be, there is an unuſual determination of blood to the part, and wherever the circulation is increaſed, there, as appears from the caſe of topical inflammation, a greater degree of heat will moſt certainly be excited.

3tio. Cold acts upon animal matter, as all other aſtringents do, by conſtricting it, and, at the ſame time, either by this conſtriction alone, or ſome leſs evident mode of action, it proves a tonick to the muſcular ſyſtem: and hence, when cold induces fever, it produces alſo that increaſed action of the circulating powers, which conſtitute the *diatheſis phlogiſtica*, or inflammatory habit. It is on this account, that rheumatiſm and inflammatory diſorders in general, are moſt commonly the produce of cold climates, and cold ſeaſons, and frequently the immediate effect of external cold. It is therefore obvious, that cold both acts as an aſtringent and a tonick. As an aſtringent, it tends to diminiſh ſpontaneous evaporation; and as a tonick to brace the moving fibres, and increaſe the vigour of the ſanguiferous ſyſtem. Such are the more general effects of cold on animated matter: and here we cannot ſufficiently admire the ineffable wiſdom of the great author of nature, who has ſo

 framed

framed the animal machine, as to render the cold, to which it muſt be inevitably expoſed, in different climates and ſeaſons, a powerful agent in obviating the deleterious effects, that we ſhould, *a priori*, have expected from it. A certain degree of cold, by increaſing the tone of the veſſels, augments their action, and thus cauſes a greater quantity of phlogiſton to be brought into an active ſtate in a given time: And hence, not only more heat is generated in the body in a cold, than in a warm atmoſphere, but alſo, as both ſenſible and inſenſible perſpiration are much leſs conſiderable, the heat excited is carried off more ſlowly; for the cold, we have ſeen, produced by ſpontaneous evaporation, is always proportionate to the celerity of the proceſs. A multitude of other circumſtances have a manifeſt tendency to mitigate the piercing cold of northern regions, and enable man to preſerve uniformity of temperature. In cold countries, his food is chiefly animal, his drink ſtrong and generous: The former affords him a conſtant and copious ſupply of phlogiſton, in a ſtate readily ſeparable by the circulating powers, and the latter acts as a ſtimulus to the heart and arteries, and thus haſtens the developement of that ethereal fluid.

fluid. *Pliny*, one of the most celebrated naturalists of antiquity, pathetically laments that, whilst nature has given various clothing to the brute creation, and even fenced plants and trees with bark, against the injuries of cold and heat, she should have cast man into the world naked, and unprovided against the inclemency of different climates, and seasons. But, instead of agreeing with that philosopher, that nature has, in this particular, acted more like a cruel stepmother, than a kind and indulgent parent to man, we cannot, in my opinion, sufficiently extol her providence and wisdom. It was no more than consistent with equity, to provide the irrational part of her works with clothing suitable to their circumstances; but man, whom she endued with the transcending faculty of reason, and intended to be the inhabitant of every climate, she very wisely left to accommodate himself to the difference of his situation, and clothe himself accordingly with the plumes, the fleeces, the skins of animals, and the product of various plants and trees. It is in some measure owing to this provident conduct of nature, that man can with impunity emigrate from the center of the torrid zone, to the most northern regions of the inhabitable globe.

globe. In the former his clothing is light, thin, and more for ornament than use; in the latter, the shaggy race supply him with their thickest furs; his habitation is well contrived to guard him from the inclemency of the weather; and large and constant fires mitigate the severity of the air.

SECT. IV.

Of the Connexion, which subsists betwixt the degree of heat, the State of Respiration, and Colour of the Blood.

In relating the phænomena of the heat of animals, we had occasion to observe, that the degree peculiar to each species is very intimately connected with the state of respiration, and the celerity, with which the air is rendered mephitick in passing through the lungs. The *rationale* of this fact is readily given. The phlogiston, which escapes from the lungs, can only be derived from the blood; and the blood, we know, of the hottest animals abounds most with red particles. It is therefore natural to suppose, that the phlogistick matter should fly off from them, in

greatest

greateſt quantity, and ſooneſt vitiate the atmoſpherical air inſpired by them; and, as we have found that to be really the caſe, we have no room to doubt that thoſe animals are hotteſt, whoſe ſyſtem is ſo framed, and functions ſo regulated, that the largeſt proportion of the principle of inflammability is evolved from their blood; a farther proof that their heat depends on this laſt circumſtance. The reaſon why the warmeſt animals breathe moſt frequently, is obvious. Reſpiration, though a voluntary function, is neceſſarily regulated by the circumſtances of the economy; and as its chief purpoſe is to carry off effete phlogiſton from the lungs, unleſs it be inceſſantly exerciſed by hot animals, they muſt ſoon inevitably periſh, by an accumulation of that deſtructive principle in the pulmonary ſyſtem.

The laſt phenomenon I ſhall attempt to explain, is the cloſe relation which the colour of the blood bears to the degree of heat peculiar to each ſpecies of animal. It was formerly ſhewn that the blood of the mammalia contains more red globules than that of the amphibia; and the blood of the amphibia more than that of the branchial fiſhes. It was likewiſe remarked, that the degree of heat in each, bears an exact proportion to the

colour

colour of the blood. How are theſe facts to be accounted for? No rational ſolution has heretofore been offered; but an attentive conſideration of the principles laid down in the courſe of this eſſay, will, I truſt, afford a ſatisfactory explanation of the problem. Various proofs were brought to ſhew that the colour of the blood depends upon the ſtate of its phlogiſton, and that it is always of a lighter, or deeper red, as there is more or leſs of this principle preſent in an evolved ſtate: If, therefore, animal heat is owing to the developement of the inflammable matter, it is plain that the degree of heat, and the colour of the blood, will be ever in an exact ratio to each other. A multitude of other phænomena ſtill remain to be explained; but as I have already far exceeded the boundaries of my intended plan, and more eſpecially, as it is hoped that they will in general, be found to admit of a ready explanation on the radical principles of the theory, which has been here propoſed, I muſt beg leave to ſubmit them, and whatever difficulties may occur, to the candour and ingenuity of the reader.

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ERRATA:

Page 297, line 25, for *conceiving* heat, *read* receiving; p. 329, line 21, for *law*, r. *laws*, &c. &c.